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Contents on Colored Page IX

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SEMI-MONTHLY.

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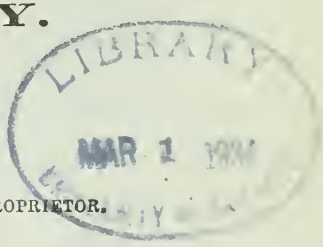

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Complete Index to Advertisements, on Colored Page xxxiii.

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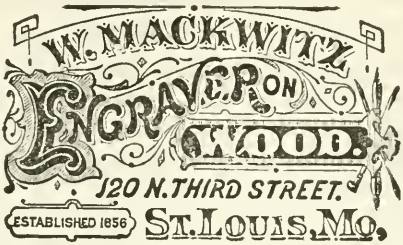

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CONTENTS.

ORIGINAL CONTRIBUTIONS.

ARTICLE. PAGE.

VI. The Influence of Foul Air. By J. E. TEFFT, M. D., of Springfield, Mo., Late President of the Medical Association of the State of Missouri..... 183

VII. Elaterium in Dropsy. By A. WISLIZENUS, M. D., of St. Louis.. 186

REPORTS ON THE RECENT PROGRESS OF MEDICINE.

Venereal Diseases By LEGRAND ATWOOD, M. D., Collaborator for the JOURNAL..... 191

TRANSLATIONS FROM THE ITALIAN.

Storia Compendiata Della Chirurgia Italiana. Dal Suo Principio Fino al Secola XIX—(Compendiated History of Italian Surgery.) Del Professore CARLO BURCI. Translated for the Journal by JOSEPH WORKMAN, M. D., of Toronto, Canada..... 195

CLINICAL REPORTS FROM PRIVATE PRACTICE.

A Case of Incomplete Strangulated Hernia, Reduced after eight days. By W. B. CRAIG, M. D., Prof. of Anatomy, St. Joseph Hospital Medical College, of St. Joseph, Mo..... 203

ROCEEDINGS OF MEDICAL SOCIETIES.

American Laryngological Association—Bucco-Pharyngeal Tuberculosis or Phthisical Ulceration of the Fauces. By F. H. Bosworth, M. D., of New York..... 205

Tri-State Medical Society—Laceration of the CervixUteri—The History and Diagnosis. By E. C. Dudley, M. D., Former House Surgeon to the Woman's Hospital in the State of New York..... 213

St. Louis Medical Society. Central Lesions in Neuralgia..... 221

A Unique Letter..... 222

On the Relations of the Placenta to Postpartum Hemorrhage by Walter Coles, M. D..... 223

Discussion..... 231

Diabetes..... 233

Removal of the Placenta..... 235

The Use of Pessaries..... 236

BOOK REVIEWS.

A System of Midwifery. Including the Diseases of Pregnancy and the Puerperal State. By Wm. Leishman, M. D..... 241

The Pathology and Treatment of Venereal Diseases. By Freeman J. Bumsted, M. D..... 243

Notes on the Treatment of Skin Diseases. By Robt. Lieving, M. D.. 244

Disases of Women. By Lawson Tait, M. D..... 244

Books and Pamphlets Received..... 245

Meteorological Observations..... 246

Mortality Report—City of St. Louis..... 246

DRUGGISTS' DIRECTORY.

MISSOURI—Continued.

Vaughan & Thom.....Bromnville
Chenoweth & Farmer.....Brownsuille
Cutler, Geo. L.....St. Joseph
Eaton, T. J. & Co.....Kansas City
Ellis & Prather.....Maryville
Gittings, M. R. (P. M.).....Carrollton
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Scholz, P. C.....Corning
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MONTANA.

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Reinhard, J.....Missoula

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ILLINOIS—Continued.

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Hobie, Louis.....Shoal Creek
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Murdick, Dr. Jacob.....Germantown
Northrop, J. B.....Winchester
Rogers & Montgomery.....Quincy
Seaman, L. S. & J. O.....Shelbyville
Wheeler, Z. S.....Galatia

GEORGIA—Continued.

Lee & Riley, Drs.....Lawrenceville

IOWA—Continued.

Brayton, Chas. J.....Dubuque

TEXAS—Continued.

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For Reports of Actual Cases See Opposite Page.

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For original articles on the clinical use of Dextro-Quinine during the year 1879, In Remittent, Intermittent and Typhoid Fevers, Pertussis, Cholera Infantum, Pneumonia, Periodic Neuralgia, etc., see communications entitled, "On the Use of Dexiro-Quinine."—MEDICAL AND SURGICAL REPORTER, January 25th. "Dextro-Quinine."—*Medical and Surgical and Surgical Reporter, April 5th.* "A Case of Pneumonia, etc., treated by Dextro-Quinine, etc.—*Medical and Surgical Reporter*, December 20t . "Dextro-Quinine as an Antiperiodic."—*Ohio Medical Recorder*, March. "On Dextro-Quinine."—*N w Remedies*, March. "Dextro-Quinine as an Antiperiodic."—*N. Y. Eclectic Medical Journal*, June. "Dextro-Quinine."—*Cincinnati Lancet and Clinic*, August. "On the Use of Dextro-Quinine"—*Louisville Medical News*, April 5th and May 17th. "Dextro-Quinine as an Antiperiodic"—*Medical Brief*, July." "Malarial Fever of the South."—*Southern Medical Record*, August. "Dextro-Quinine in Pertussis."—*Southern Medical Record*, November. "Dextro-Quinine."—*Medical Summary*, (two articles) October. "Notes on Hospital and Private Practice."—*Pacific Medical Journal*, October "Dextro-Quinine."—*Western Lancet* (San Francisco), December.

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FACTS FROM MISSOURI AND ILLINOIS!

THE USE OF DEXTRO-QUININE IN INTERMITTENT FEVER

85	J. W. D., male. *Quotidian.*	63	3	0	2 grs. every two hours.	14 gr.	It was a case of slow fever, which had terminated in a quotidian intermittent. Patient had taken quinine at times for the past 35 years. No unpleasant symptoms or ringing in the head from this *Dextro-Quinine*. N. B.—This patient was opposed to quinine, as he insisted that it always produced unpleasant symptoms and ringing in his ears, but he admitted that he experienced no unpleasant symptoms from the *Dextro-Quinine*.	T. G. Comstock, M.D., Good Samaritan Hospital, St. Louis Mo.

After a year's experience with DEXTRO-QUININE, Dr. T. G. Comstock writes us under date of November 21st, 1879, as follows:

"I take great pleasure in making known to you a general report of my experience with your preparation of Dextro-Quinine. I have not kept accurate reports of the cases where I used your preparation, but I have, for the past year, employed it in several hundred cases, of all forms of diseases, complicated with paroxysms intermitting in character, such diseases depending upon malaria as a primary cause or aggravation. I employ it almost daily, in not only clear cases of intermittents, but especially in Neuralgias. I have by no means abandoned the use of sulphate of quinine, but in a large experience I have found your Dextro-Quinine to take the place of the sulphate in more than one-half of the cases where I previously should have given the last named preparation. The Dextro-Quinine produces much less 'tinnitus aurium' than the sulphate, and the majority of patients do not experience any ringing of the ears whatever. I am very glad to take this opportunity to express to you my greatest confidence in the efficacy of the new preparation as a reliable antiperiodic. It has, for some months past, been frequently prescribed by the attending physicians of the Good Samaritan Hospital in this city, and the other members of the medical staff of the hospital as well as myself, have confidence in its efficacy.

Yours very respectfully,

T. G. COMSTOCK M. D."

102	James C., male, *Tertian Ague.*	17	3 months	0	20 grs. in 5 gr. doses during intermission.	20 gr.	Had taken cinchonidia repeatedly but it kept returning. Severe tinnitus aurium. No return in six weeks.	D. H. Chase, M., D., Hoosier Prairie, Clay county, Ill.
103	Mary W., female. *Tertian.*	10	5 months	0	15 grs. in 4 doses day before the paroxysm.	25 gr.	Did not complain of quinism. No return in 4 weeks.	D. H. Chase, M. D., Hoosier Prairie, Clay county, Ill.
104	J. M., female, *Quotidian*	3	3	0	6 grs. in 2 doses during intermission.	6 grs	Was active determination to brain and spasms during the fever; but the *Dextro-Quinine* did not affect the head. Well now for six weeks.	D. H. Chase, M. D., Hoosier Prairie. Clay county, Ill.
105	M. Carragan, male.	43	2	0	3 grains every 2 hrs. until 3 doses only were given on the evening before the coming chill in the morning.	9	After taking the second powder he began to perspire freely. Could not stand Sulph. Quinine. No unpleasantness from the DEXTRO-QUININE. This case was treated by the sample sent to me by mail. It worked like a charm. Shall give it a fair trial.	H. L. Cokenower, M. D., Pleasant Plains Ill.
123	William Tannahill.	32	Every 7 days for about one year.	One slight symptom. Is now well.	I gave it in 5 gr. doses allowing patient to take 6 doses day before chill day, then 1 two gr. pill every day for 6 days, and repeat former dose as before. This was done for 28 days.	120 gr.	Patient was reduced physically by the long recurrence of these paroxysms so that his life had almost become a burden. His digestive powers were *markedly* compromised. He had been treated by all the Cinchona alkaloids without arrest of the paroxysms.	E. Newton Campbell, M. D., Walnut Grove, Illinois.
161	W. H. male Had tried everything without success.	35	Many & constantly for six months	1	20 grains in six powders, 1 every 2 hours. Commencing after the paroxysm.	30 grs	Patient has had ague for at least six months; said he had tried almost everything—was entirely discouraged. It has been two months since the last of the *Dextro-Quinine* was taken. No return of the paroxysms and "Richard is himself again."	E. Newton Campbell, M.D., Walnut Grove, Illinois.

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SAINT LOUIS

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Original Contributions.

ARTICLE VI.

THE INFLUENCE OF FOUL AIR. By J. E. TEFFT, M. D., of Springfield, Mo., Late President of Medical Association of the State of Missouri.

The *Boston Journal of Chemistry*, states (Dec., 1879) editorially, that "There is no proposition so axiomatic or self-evident that a German cannot be found to dispute it." To show that to be true it cites the remark of Hebra, that many people bathe too much, and particularly to the recent experiments of Dr. Emmerich, of Munich, upon himself and others, tending to show that the drinking, whether in health or disease, of foul-smelling and putrid water is by no means so injurious as has been supposed, if indeed it be injurious at all. The *Journal* in conclusion says "What 'learned Dutchman' will now come to the defence of foul air? If the waters of the ditch can be proved wholesome, why not the gases of the sewer and the cesspool?"

The Boston editor has doubtless forgotten the fact that a "Learned Dutchman" has already expressed an opinion, not perhaps quite so strong as desired, but at least tending in that direction. Wagner (General Pathology Am., ed. p. 65) says:

"Although it is certainly more healthy to breathe pure air, yet it cannot be said that definite diseases are produced by air made impure by human exhalations." My present object is simply to detail some facts which came under my own observation, which tend to show that Wagner is not far wrong; that the inhalation of foul air does not necessarily and uniformly produce disease and that seems to set at defiance the best established and most generally accepted laws of hygiene.

In the year 1874, I was physician to the Greene Co. (Mo.) jail. The jail-building is a two-story brick structure, forming a wing on the rear of the County Court House. Running east and west through the center of the building, is a corridor about six feet in width, having no ventilation at night, and in daytime ventilated by a grated door at either end, the east end door opening into the basement of the Court-House. There is also a narrow hall from the center of the corridor, south to the street, the hall door being generally closed. On the north side of the corridor are three cells having grated doors and a small north window ten feet from the floor and about thirty inches square. Thus it will be seen that the ventilation of these cells is extremely imperfect.

In one of these cells is an iron box made of boiler iron about 8½ feet high. The door is solid excepting a small slide five feet from the floor large enough to hand in provisions and water to the inmates, perhaps six by twelve inches. On the top, there are four longitudinal openings running across it about three inches wide. Having no openings at the bottom, those at the top establish no current of air. Here we have a nearly air-tight box, in a cell with but very little ventilation, opening out of a corridor which has but little more. Yet the worst is to come. In a back corner of the *box* is a privy seat with a large iron discharge pipe curved in a quarter circle passing through the floor, out through the foundation of the building and terminating in a large covered privy vault outside, which outside privy was in constant use and in which disinfectants were not used. When the lid was raised, as it must have been many times a day, a horrible stench came up through the tube, filled the box, the cell, the corridor, the other cells, the hall, and I have often smelled it outside of the front door of the building.

The cubic contents of the box is 1120 feet, making no allowance for the bunks, the mattresses, the bedding and the bodies of the inmates.

In cold weather a small stove burned in it, and for perhaps half of the twenty-four hours, a coal oil lamp. Yet there were confined in that box during that year, never less than six, and at times as high as twelve persons.

Hygenic authorities insist that in prisons, hospitals or schools there must be eight hundred cubic feet of air for each person, unless the means of ventilation are extraordinary, and if so not less than six hundred feet. In this case when the crowding was the greatest, there was as low as eighty, and when there were six inmates not over one-hundred and fifty cubic feet, after making the above allowances, and no ventilation at all, except from the privy vault.

On looking over the situation I went before the County Court and made a pathetic speech, gave a scientific exposition of the subject of ventilation, and declared it to be almost certain death to any human being to remain there during the hot weather, etc. [The experiment had not been tried as the box had just been made]. However, the Court did nothing.

One may judge of my surprise, when I say that during that year and the next, while I remained in medical charge of the jail, the health of the prisoners was excellent. There was not a case of fever or diarrhœa during those years which originated in that cell.

There was confined there one man, a defaulter, a man of luxurious habits, accustomed to every comfort and convenience, but in impaired health. His attending physician, the late Dr. Shutt, told me he had albuminuria. I expected fully that he would die speedily from the foul air and confinement. Yet during the hottest of the summer he enjoyed excellent health, and in the few months he was there, he gained twenty pounds of flesh. He took no medicine beyond an occasional vegetable laxative pill. During the same summer an old man of 60 years was confined on a charge of murder. At his capture he received a gunshot fracture of the radius and a severe flesh wound in the shoulder, one in the hip and one in the thigh. Here, surely, I thought, I will have erysipelas, septicæmia, and the compound fracture will never unite. Yet I am obliged to say that no wounds ever healed more kindly.

The cell is in the same condition still, only a ventilating chimney has been carried from the top of the cell to the roof of the building, the privy arrangements remaining the same. Yet

the inmates have enjoyed (though perhaps not so crowded), uniform good health, as I am to-day (Jan., '80) informed by Dr. Flanner, the attending physician.

"One swallow does not make a summer," yet the experiment has been long enough—now seven years—to demonstrate that something besides stink and foul air is necessary to make people sick, or at least to raise the question whether those agencies act in this direction as uniformly and as certainly as we have been accustomed to think they do.

ARTICLE VII.

ELATERIUM IN DROPSY. By A. WISLIZENUS, M. D., of St. Louis.

There is, perhaps, no disease that requires so much individualization in its treatment as general dropsy. Effusion of water in serous membranes or in the cellular texture, is but the symptom of many different diseases, as, for instance, organic diseases of the heart, liver, spleen and kidneys, inflammations, abnormal blood, malarial infection, rheumatism and gout, disturbed equilibrium of importation and exportation in the system, mechanical impediments to the free circulation of the blood, nervous prostration, etc. It follows, then, that general dropsy requires very different treatment according to the main cause and according to the constitution and surrounding circumstances of the patient. We possess, therefore, no specific suited to all cases of dropsy, and the various remedies recommended in this disease are too numerous to be mentioned here. One of the oldest, known already to Hippocrates, is elaterium, the exuding juice of the ripe fruit of *momordica elaterium* Its action is powerfully hydragogue-cathartic-diuretic. In the 17th and 18th centuries it was especially used and recommended by Sydenham and Boerhave. They performed wonderful cures with it in this disease where all other remedies had failed, because as acute observers they individualized and reserved their grand remedy for suitable cases. But their followers, who regarded it as a general specific in this

disease, soon brought it into discredit by indiscriminate application and by indiscreet doses, and in course of time it was almost forgotten, and became nearly obsolete. In this century it has gradually been revived by various physicians in different countries, who use it with proper discrimination, and to great advantage. In my own experience, I have often had occasion to give a trial to this remedy in obstinate cases where it seemed to me fully indicated, and it proved generally successful, sometimes even beyond my expectations; and since it is not as generally appreciated as I think it deserves to be, it may not be amiss to report a case that lately fell under my attendance—a very typical case, illustrating clearly the indications for the remedy.

The patient was a German, 56 years of age, a strong, stout man, fat but yet muscular, and weighing 225 pounds. He had led an active life and was not addicted to liquors, drinking mostly beer and light wines. About a year ago, he began to feel slight dyspnœa in mounting stairs or walking fast. Regarding this as a disposition to asthma and being habitually constipated, he relieved himself temporarily by cathartics, but gradually his feet began to swell, his dyspnœa increased rapidly, and early in the winter, he was confined to his room and bed. When called to his assistance, I found him sitting in an arm-chair, the easiest position for him on account of his dyspnœa. Examination of the chest showed dilatation of the heart and in the præcordial region such a discord of sounds that it took me some time to analyze it into a feeble, unrythmical, sometimes intermittent systolic and diastolic murmur, muffled as it were by promiscuous and irregular sounds of regurgitation, proceeding from valvular insufficiency in the pulmonary artery and aorta.

Both lungs were emphysematous and exhibited loud mucous râles everywhere, but especially in the left lung, where the râles expanded fanlike, as if some embolus obstructed the right branch of the pulmonary artery; the murmur of respiration was not quite suppressed, but bronchial; a constant cough with very little expectoration troubled him; lying down or sleeping was out of the question. His extremities were quite cold; the pulse at the wrist could scarcely be felt, was slow, feeble, and sometimes intermittent; his liver was somewhat enlarged and of torpid action; urine very scanty, with thick sediment of uric salts, but no albumen; abdomen in the lower part filled with water; anasarca in the lower extremities to a very high degree.

It was apparently a case of general dropsy from disease of the heart, prognosis very bad. The patient had never been affected with rheumatism or gout, nor in fact with any serious illness before. The only cause to which he could attribute his present disease, was that in his business he had a great deal to do with ice and ice houses, and that during the summer he often drank very cold beer. Evacuating medicines and promotion of all the secretions were certainly indicated, and as elaterium is especially useful in dropsies from heart disease, I would have tried it at once if the patient's extreme weakness had not forbidden it. I ordered the patient therefore to bed, placing him in a sitting position, had his feet warmed, gave him some wine with Vichy water, and later cathartics, such as Hunyadi Janos, common drastics, sometimes with calomel, sometimes with quinine; between them diuretics, of squills and acetate of potassa; rubbed the extremities with camphor and bandaged them; used iodine externally on the chest and abdomen, etc. I allowed him, also, a nourishing diet and light wine. Continuing this treatment about two weeks, I perceived no real improvement in the objective symptoms except that the patient felt rather stronger and the circulation of the blood was somewhat freer. This appeared to me the right moment to give the elaterium a trial. His naturally strong constitution, the integrity of his digestive organs and his former tendency to contipation gave me confidence that he would bear a more heroic treatment and that the elaterium was now fully indicated. I commenced my new treatment by prescribing elaterium in the following formala:

℞. Elater. (anglic. alb.).....gr. j.
P. extract. coloi comp.... .....3j.
P. extract. hyose. sicc.....grs. xji
M. exat tissime
Mutit. gum. arab. q. s. ut. f. pilulæ.....No. xij.
Ds. 1 pill to be taken every night.

The first pill acted like a charm on my patient. It produced within a couple of hours at first some griping pains and then such copious and powerful action of his bowels, both fæcal and watery, that towards morning he felt quite weak, but so relieved in his chest, that for the first time since many weeks he enjoyed a good sleep for several hours. The daily course of treatment, medicinal as well as dietetic, which I now prescribed for

him, was the following: When the effect of the pill was nearly over, he took a cup of strong and warm beef tea, slept then a couple of hours, then breakfasted on eggs and coffee; later, a dinner of some roast meat with light vegetables, as a common drink using Vichy water with some light wine. But in the afternoon he had to return to medicine, to a diuretic mixture, the decided and good effect of which I have on so many occasions observed, that I may be excused for communicating its formula:

R. Potass. Carbonate pur. (e Tartaro).....ʒ ss.
Acet. scillitic. q. s. ad perfectam saturationem
Tartr. potass. boraxat.....ʒiij.
Spirt. nitr. æther .....ʒj.
Aq. destill. q. s., ut fiat mixtura.....ʒvij.
Oxymell. scillit..... .....ʒj
M Ds. S The fourth part to be taken.

Of this mixture, patient took every afternoon or evening one-fourth, and later, when I saw his stomach could bear it, one-half, and it produced a marked effect, not only in clearing the urine in a few days from all sediments, but also in increasing its quantity considerably. The external use of iodine was also continued, but to a greater extent, by brushing alternately with the tincture one day the whole abdomen, and on the following the lower extremities. The good effect of this combined treatment, the main pillar of which was after all the elaterium, was so conspicuous, that from day to day the objective symptoms of the disease receded more and more, so that after two weeks I regarded the patient as convalescent. All dropsical symptoms had disappeared, kidneys and liver in normal action, digestion good; and in the chest instead of the emphysema and râles, the natural murmur of respiration pervaded the lungs; no more cough nor dyspnœa. The heart was still somewhat dilated, but no more regurgitating sounds were heard; systole and diastole worked in regular rythm; pulse from 60° to 70° and the only remnant of the former valvular insufficiency I discovered in the casual, momentary retardation of the systolic contraction, occurring only at long intervals. But of all these important changes the patient himself appreciated most highly, the sound sleep which he now enjoyed in all positions and at all hours of day and night, and least of all he regretted the loss of twenty-five pounds in his bodily weight.

To prevent a relapse, I continued the remedies still for some weeks in reduced doses; and now that he is up again and attending to his business, he takes only every morning a tumblarfull of Hunyado Janos, and in the evening, a tablespoonfull of diuretic mixture, and avoiding all the other stimulants he drinks only Vichy water with a moderate quantity of light wine.

As elaterium is often found of very different quality, the physician who prescribes it ought himself to examine the preparation on hand in the drug store. The most reliable is the so called English elaterium, white-greyish, in small light tablets or fragments, containing the substance deposited spontaneously by the juice of the fruit. Another preparation is gained by expression of the seeds and other parts of the plant, and forms a black extract. The first one acts in minute doses; I begin generally with one-twelth or one-tenth of a grain as a dose. The latter is most irreliable in its action and has to be given sometimes in doses of one or two grains. It is therefore safer never to prescribe the black extract of elaterium. In compounding pills of this medicine, the minute dose of elaterium should be so thoroughly and carefully mixed with the other ingredients, that every pill will contain an equal quantity of it. In every case the pills should be freshly prepared.

Reports on the Recent Progress of Medicine.

VENEREAL DISEASES.

By LEGRAND ATWOOD, M. D., Collaborator for the JOURNAL.

FRENCH VIEWS OF SYPHILIS.—A French Journal, *L' Année Medical*, Caen, has in its last few issues had some excellent lectures on syphilis by Dr. Denir-Dumont, surgeon-in-chief of the Hotel-Dieu of that city. In concluding his last one, he says: "These three conclusions contain the result of our study of syphilis — a *unity* of origin of all syphilitic maladies; the *incurability* of the disease, when the immense variety of its effects is considered; and as a logical consequence an indefinite *continuance* of treatment, with occasional interruptions.

These discouraging views are those now generally prevalent in France. They are forced upon surgeons by constant discoveries of hitherto unobserved connections of pathological changes with previous syphilitic poisoning; and probably, these discoveries are by no means complete.—*Med. and Surg. Rep.*, Jan., 1880.

SYPHILIS IN RUSSIA.—Syphilis, says Dr. Podolinski, is the principle scourge of the rural population of nearly all of Russia, but its ravages are greatest in the South, in the government of Kiew, Poltava and Theringnon. In some of the villages a third of the inhabitants are contaminated. In Taroslowka, of 120 families, 30 are certainly syphilitic, and 64 only are known to be healthy. The influence of the disease on the degeneration of the population and the increase of mortality is very great. There is scarcely an example of a member of an infected family having passed the age of sixty, and the death rate among the syphilitic is more than one-half greater than that of the remaining population.—*Med. and Surg. Rep.*, Dec., 1879.

SYPHILIS, ITS INFLUENCE UPON THE COURSE OF WOUNDS.—Dudterhoff (*Wien. Med. Woch.*—*Bost. Med. Jour.*,) summarizes the results of experience derived from the recent wars in Europe, as to the action of the syphilitic poison upon wounds received by its possessor, thus: (1) Superficial contusions or continuous irritation

of the wounds, can during the contagious period of syphilis, cause the appearance of syphilitic efflorescence at these points of irritation, without however, influencing the healing of the wound. (2) Wounds received close to or touching the primary induration may yet heal *per primary*. (3) Latency of syphilis is favored by the increased activity of tissue metamorphosis during the healing of severe wounds, but as cicatrization becomes complete, the syphilis may appear at the point of injury or elsewhere. (4) Old syphilis, if latent, does not interfere with union by first intention after surgical operations; if present, the poison acts unfavorably. (5) Bone syphilis predisposes to fractures and militates against consolidation. (6) Inveterate syphilis, with diseased bone and general exhaustion, may cause wounds to assume a definite form of gangrene, yielding to specific treatment. (7) Constitutional syphilis has no connection with pyæmia, nor is it proved to predispose to bleeding of wounds.—*Detroit Lancet*, Dec., 1879.

Dr. Burkley, of New York, treats chancre with ½ grain of green iodide of mercury, morning and night.—*Exchange.*

Dr. Burkley reports, in *Archives of Dermatoloy*, October, 1879, two cases of syphilis supposed to have been contracted by means of cigars. The subjects were both physicians of intelligence and education, who had devoted much time to the investigation of their cases, and excluded other means of contagion. Both had chancres on the lips, which were followed by constitutional manifestations, relieved by specific treatment. He suggests as a prophylactic to those who use cigars, the use of the cigar-holder.—*Margland Med. Jour.*

Treatment of Gonorrhœa in Women.—It is not the simple matter one would be led to suppose upon reading the descriptions given in the majority of text books. Injections prescribed, such as the zinc, alum, copper and silver salts are very slow as well as insufficient in their work. The virus not only invades the genital folds of the vulva, but likewise the urethra, vagina, cervix uteri and even the uterine cavity and oviducts. I know of nothing comparable to the excessive use of hot water douching in the initial symptoms of the disease, as the retention of the acrid virus in the folds of the vagina is apt to beget erosions of the mucous surface which rapidly undergo ulceration. Any method to prevent friction of the vaginal walls is beneficial and as the

common clay found in Pennsylvania, Missouri and other States, is a most admirable antiseptic as well as very soothing to inflamed surfaces, my practice for some ten years past is to thoroughly irrigate the vagina with hot carbolized water. Place the patient in the knee-chest position, retract the perineum with the Sim's speculum, and after the vagina is ballooned by asmospheric pressure to thoroughly dry the surfaces with cotton, then paint them with a saturated solution of nitrate of silver, followed by a solution of common salt, to make an insoluble silver chloride to prevent too much cauterization; then to dry the surfaces once more with the cotton and then fill the vagina with dry powdered clay or a magma of clay, vaseline or thymol. The clay is not only an antiseptic, but it protects the mucous surfaces from attrition, and is at the same time a pessary-mould on which the uterus rests. Besides these advantages it absorbs all of the gonorrhœal discharges and keeps the genital tract clean and free from smell. Twenty-four hours subsequently the patient can wash everything out of the vagina by means of the hot water douche and you can re-apply the nitrate of silver, etc., again. By this mode of treatment you will cut short an attack of gonorrhœa, and will save your patient much subsequent distress which ensues after the ordinary injection plan as laid down in the books, because you free the crevices and folds of the vaginal surfaces of all contagious matter, and you cauterize the cleansed mucous membrane instead of precipitating and balling the mucosities to be retained and rub the excoriated surfaces. With regard to internal medications I know of no specific, even when there is gonorrhœl urethretis, other than some demulcent drink to render the urine bland and unirritating. Copaiba, cubebs, sandal-wood oil, etc., are worse than useless, because they not only do no good but they are frequently productive of harm by upsetting the patient's digestion.—Montrose A. Pallen, A. M., M. D., *in Gaillard's Med. Journal*, Dec., 1879.

ON THE TREATMENT OF SYPHILIS WITHOUT MERCURY.—In the January, 1880, number of *Braithwaite's Retrospect* is a communication, taken from the *British Medical Journal*, July, 1879, under the above caption. It is from Dr. J. P. H. Boileau, B. A., F. R. C. S. I., Surgeon-Major, Assistant Professor of Pathology, Army Medical School. The leading idea of the writer, although not original, is an important matter for consideration at the

hands of the profession, and it is to be hoped that further observation of the treatment of syphilis without mercury may be made by careful practitioners. Admitting the correctness of the views of Keyes, of New York, that, administered in the manner described in his work, mercury is a decided tonic and that under a course of treatment extending over a space of three years, patients improve in strength and health and even beg the privilege of continuing the treatment upon the ground that they never enjoyed such health before. Still, if the disease can be effectually eradicated without the use of so potent a drug, it would certainly be better to discard it altogether. Dr. Boileau says: "My position is briefly this. In May, 1865, I was gazetted to a regiment about to proceed abroad. I served with it in the Mediterranean, in Canada and in the West Indies and at home; and left in October, 1876. For ten years of that period I was most closely associated with the regiment and had many facilities for prosecuting injuries into the results of the treatment of disease, and, by observations, continued year after year, for satisfying myself as to the state of my patients long after they had been the subjects of my treatment, and so I am in a position to-day to bring to your notice not only cases of syphilis treated without mercury, but I believe cases of syphilis cured (by natural processes or otherwise) without mercury.

The principle, which in the main guided me in the treatment of syphilis, was the principle of avoiding mercury as much as possible, and in carrying out this principle I treated most of the cases without prescribing any form of that remedy. Rarely did I resort to fumigation or inunction, and I now believe that it would have been better had I discarded all local remedies having mercury in their composition. As a rule, I did not use such applications. I tried the internal administration of mercury in some cases without being at all satisfied concerning its alleged antidotal or curative powers."

He then outlines the history and treatment of seventeen cases, in not one of which was mercury administered, and yet each one was clearly and unmistakably true syphilis, in which characteristic secondary manifestations followed indurated chancres without suppurating buboes. In conclusion he says: "I would beg of those who hold that the specific action of mercury is absolutely necessary for the eradication of syphilis from the organism to consult the works of Hughes Bennett, Lanceraux,

etc., for a list of observers, committees or councils that have declared in favor of the treatment of syphilis. My experience as an army medical officer of fifteen years' service, ten of which were passed with one regiment, compels me to believe that syphilis is as curable without mercury as is small-pox or typhoid fever. And until I have seen better proof than a mere *ipse dixit* that all the patients that I have treated without mercury are still under the influence of the syphilitic poison, I certainly cannot admit it. To the logic, however, of well-ascertained facts, I am quite vulnerable."

Translations.

FROM THE ITALIAN.

STORIA COMPENDIATA DELLA CHIRURGIA ITALIANA. DAL SUO PRINCIPIO FINO AL SECOLA XIX. Del Professore CARLO BURCI.* [For the JOURNAL.] JOSEPH WORKMAN, M. D., Toronto, Canada, Translator.

COMPENDIATED HISTORY OF ITALIAN SURGERY.

The above is the title of a most erudite and interesting article which, at the request of the editors of a new Dictionary of the Medical Sciences, then about to be published at Pavia, was, in 1872, furnished by Burci, but owing to the undue extent it had reached, it was found too voluminous for the available space at the command of the publishers, and was, in consequence, reluctantly declined.

Not long afterwards Burci was gathered to his fathers. Whether the mortification induced by the rejection of a production which had cost him so much toil and patient research contributed to the shortening of his days, is a question which must best be solved by those who have encountered similar disappointments. It is impossible to read this "Compendiated History of Italian Surgery" without admiration of his comprehensive

*Published in the proceedings of the Royal Institute of Superior Studies, Medical and Surgical Section, Florence, 1876.

knowledge of the subject on which he had been invited to write, or without profound sympathy with the patriotic ardor by which he seems throughout to have been inspired, even though we may at times feel inclined to doubt whether his perpetually recurring exhibitions of zeal in vindication of the paramount claims of his native country to scientific eminence might not have been profitably held in check. An old proverb says, "Good ale requires no broom," and so think we as regards the history of Italian surgery and Italian anatomy. The facts might well have been left to speak for themselves and for the enduring fame of the men who gave them, not to Italy alone, but to the world.

To reproduce, in the pages of the ST. LOUIS MEDICAL AND SURGICAL JOURNAL, the whole of Burci's history of Italian surgery, compendiated, as indeed it is, would be a misappropriation of our space as foreign to our editorial responsibility as we apprehend it would prove unsatisfactory to our readers, the great majority of whom must be more concerned in acquiring information on the modern improvements and discoveries of their science than in acquiring a critical knowledge of its early history. We must therefore content ourselves with a mere series of abstracts, exhibiting the most important and interesting events in this department of scientific history, and signalizing the eminent contributors by whose devotion and labors it has been raised to its present honorable and universally honored distinction.

Burci has divided his history into three periods, the first of which extends from the origin of surgery down to the year A. D. 1163; the second reaches from the latter date down to the beginning of the present century; the third would appear to have been merely in contemplation, or to have been but inceptively touched upon in the winding up of the second. The work had already grown upon the author far beyond the size within which he had hoped to confine it, or to which the editors of the Dictionary had enjoined him to limit it; and as he tells us at the precipitated close of the present *brochure*, the continuation "would *merit*, not one, but several volumes," and that several eminent Italian writers are at present actually engaged in the work, we may feel reconciled to the privation, well assured that the revived spirit of Italian nationality will not suffer the scientific achievements of the proud peninsula to pass unheralded.

From Burci's first period we select only three passages, which,

as they are the records of the three most distinguished fathers of medicine and surgery, must be interesting to every lover of medical literature:

HIPPOCRATES.

"Hippocrates was born in the island of Coos, in the eightieth Olympiad, 460 years before the Christian era. In the eighty-fourth Olympiad he had become famous among the Greeks, and his works, which constitute the foundation of medicine, established the cardinal documents of the healing art, and all the best verified practices of surgery. From his writings, but especially from his "Aphorisms," his "Prognostics," his "Propositions," his offices of medicine, etc., and from his treatises on fractures and dislocations, we may well perceive the state in which surgery then was, and what the character of its instruction was. From these writings it is made manifest to every one that the art had greatly prospered, and that it had been enriched by many pathological studies and many operatory and mechanical means, calculated to cure and subdue numerous human infirmities.

In his book, "De Officina Medica et de Medico," we find recorded excellent precepts on the instruments which the surgeon ought to employ, the places suitable for oprations, the method best adapted to their execution, and on the form and uses of bandages, which were then very ingenious and special. It was Hippocrates who taught the curative system for adoption on inflamed parts; who distinguished tumors as warm, phlegmonous, and cold or pituitous; he taught the curing of abscesses, wounds and ulcers, advising at one time the application of refrigerants and emollients, and at another, of tonics and excitants, and recommending for wounds their immediate reunion, aided by proper diet and purgatives. He divided wounds into those essentially mortal (those of the brain, the spinal cord, the small intestines, the diaphragm, the urinary viscera, the heart and the large blood vessels), and into those curable, ordering hemorrhage to be arrested by compression, and sometimes by the actual cautery. He taught the practice of bleeding from every vein, and rendered common the use of dry and wet cuppings.

It is in the Hippocratic aphorisms we find it written that "the disease which cannot be cured by the knife may be healed by fire, and that which fire cannot cure is incurable." Hippocrates studied with great care wounds and fractures of the head,

not overlooking the injury now known as *contre-coup;* he advised to avoid the sutures, when trephining, and explained the operation, and he treated of the diagnosis and prognosis of these diseases with much wisdom and great learning. He prescribed the cure of nasal polypi, and of ranula; of empyema and hydrothorax by puncture of the thorax, and of ascites by abdominal paracentesis; he recommended the amputation of diseased breasts, and declared cancer incurable. He was truly a sovereign master in teaching the best means for discovering, replacing and curing fractured bones and reducing dislocations; he next treated of fistulas, renal abscess and diseases of the rectum, and bestowed much attention on everything relating to the diseases of women. In his treatise on fractures and dislocations it is manifest that he must have had knowledge of the human skeleton and of anatomy, for without this aid he could not have given such useful and appropriate instructions. Even in those early times there were *specialists* for the treatment of several diseases—lithotomists, obstetricians, etc.

Surgery having, through the labors of the Asclepiadi, of Hippocrates and his disciples, and of the *free* class, become a common art, no longer empirical or sacerdotal, but guided by rules dictated by learning and experience, and comforted by the aid of medicine itself, appeared to be destined to make prosperous advance whenever the men and the times might be propitious.

But if we except Diocles, of Caristo, who, during the lifetime of Hippocrates invented an instrument for extracting arrows, and Thesalus and Polybius (the one the son and the other the son-in-law of Hippocrates), who diffused and amplified his works, and that Prassagora, of Coos, who advised and performed gastrotomy for the iliac passion, as well as the suture of wounded intestines, no other name worthy of note is known in the interval between Hippocrates and the school of Alexandria. The grandeur of Rome by degrees attracted the most illustrious men of the school of Alexandria in every branch of knowledge, and of course in that of medicine, which now declined in the country of the Ptolemies, as much as it spread and increased in the Roman republic, where, before, physicians had been ignorant and despised, but where continual wars should at least render surgeons practical and able.

"Arcagatus, a Greek physician, came to practice in Rome two

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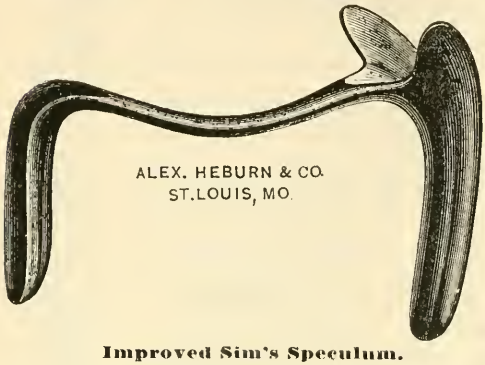

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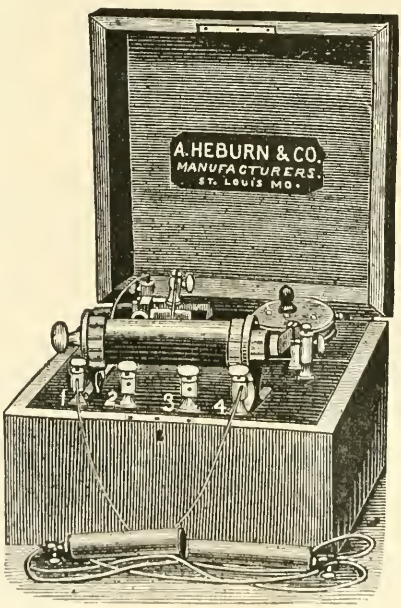

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centuries before our era. He was called the *hangman* (*il boia*), because he used in his cures both iron and fire.

CELSUS.

"The writer who gives us an exact account of the state of medicine and surgery in Rome is Aulus Cornelius Celsus. He was an Asclepiadan empiric, and was styled the medical Cicero and the Roman Hippocrates. He flourished in the time of Augustus, the golden age of Roman literature. He wrote largely *De Artibus*, and especially (in eight books) *De Arte Medica*, the only work of his that has come down to us. He was a most acute writer and critic, though he did not practice the healing art, which would have compromised the dignity of a Roman citizen! Yet in these eight *Celsian* books, how many useful precepts and sound instructions for the cure of diseases and for the practice of the art are found registered? Setting aside those which relate to medicine pure, and only rapidly glancing over what relates to surgery, I shall say that Celsus discourses masterly of wounds, ulcers and abscesses; of the method of restraining hemorrhages, even with the double ligature and the intermediate division of the artery; of the cure of poisoned wounds by means of ligature of the limb; of the sucking out of the poison, and of scarification and fire to destroy it. He treats of the extraction of spears and fixed foreign bodies; of fistulas, resection of carious ribs, amputation of gangrened limbs by cutting above the dead parts; of cistotomy, which still retains in the schools its name of Celsian, and which I have, in my lectures, shown to be the method of the bilateral cutting. Even now, who may not with profit read in this golden book of Celsus where it treats of the diseases of the eyes, and of the cure of cataract in its commencement, and of the proper operation by depression of the crystaline lens; of the extirpation of pterigium; of the incision and cauterization of lachrymal fistula; of the cauterization of the ciliary bulbs for trichiasis; of the operations for palpebral prolapse, ectopion and entopion, as well as that for staphyloma, by means of ligature or the excision and cauterizing of the ocular tumor?

"It is Celsus who, following his predecessors in the art, advises the operation for hare-lip, and that for ranula by extirpation; also the excision of swollen and indurated tonsils, and finally the extirpation of the gullet; and it is Celsus who gives

the first and most salutary precepts in autoplasty for the restoration of the nose and lips, thus leading the way to the operations of Tagliacozzi and those able masters who afterwards so amply illustrated this noble part of restorative surgery. He treats of the cure of reducible hernias, both umbilical and inguinal, recommending for the former, after accurately replacing the viscera, in some cases, compression by means of wooden pads so long as gangrene does not threaten; in others, ligature on the mass or cauterization; and for the latter the extirpation of the hernial sac, sparing the testicle, and advising closure of the supposed rupture of the peritoneum by suture. For strangulated hernia he believed baths and emolient plasters alone proper. For wounds of the large intestines he regarded enteroraphy as useful, but not for those of the small intestines. The precepts are wise which he gives for the cure of phimosis, paraphimosis and ulcers of the penis; for scrotal hydrocele, by cutting and the use of stimulating lotions of water, salt and nitre, and by semi-castration or injection, etc. It is to him we are indebted for the first observations on blood extravasations within the cranium; for special instructions on fractures in general, on their simple and complex forms, and on the opportune time for their reduction, now instantly, or again consecutively, according to the state of the neighboring soft parts.

"Celsus was the best educated and the most erudite surgeon of his time; he was exact and precise in his description of diseases, and in his search for their seats, and his inquiry as to the habits of the patients; he was rigorous in his judgments of the qualities of remedies and operations. Living in the midst of a throng of medical impostors, who made a most vile trade of their art—among specialists of every sort, charlatans, destroyers of health and traders in therapeutics, who had come from the school of Alexandria to Rome—he fearlessly preserved his independence, and in the name of Hippocrates restored to bloom the Greek medical doctrines, and brought the healing art into a highly improved state. In short, he was, as De Renzi says, "a superior spirit, who perfectly understood the wants of science and made generous efforts to meet them."

During the interval between Celsus and Galen, which comprehended a period of a century and a half, surgical art made little progress, and the men who represented it, guided by that

spirit of empiricism which then ruled in the Roman medical school, all in their turn struggled to magnify their remedies. They were mostly vulgar fellows and quacks, who looked more to their own gains than to the honor of art.

CLAUDIUS GALENUS.

Claudius Galenus was a man of marvelous genius. He was born at Pergamus, a city of Asia Minor, in the year A. D. 131. He learned the healing art in his own country, in a priestly temple dedicated to Æsculapius. He afterwards went to Alexandria to follow up his studies in the famous schools of that city, under highly reputed philosophical and medical masters, and to continue his anatomical work under Quintus. He next traveled in the East, and in a small ship sailed around all Licia in order to become acquainted with certain remedies then vaunted as excellent. Having in the course of ten years terminated his studies, he returned to his own country, where he became director of that gymnasium from which he had gone out as a disciple. But his expanded knowledge could not submit to confinement in such narrow bounds, and after two years he was induced to visit Rome, where high honors and fame awaited him, which, after his death, had become so universal as to give his name to that period of medicine, to rule through many centuries in the schools as the peer of Aristotle, of whom he was a follower and admirer.

Galen was a physician, a philosopher, an anatomist, and an experimental physiologist, studying particularly the office of the nerves, the acts of respiration, and the formation of vocal sound. He practiced surgery, but still more medicine. His scholastic method ill conforms to the free teachings of Hippocrates, and his tendency to poly-pharmacy, which he imbibed from the Alexandrian medicine, rendered him endeared to the Arabs whilst they ruled over science and art, which reached up to the Italian revival. The works of Galen were numerous, but they have not all come down to us; they constitute the most noble monument of ancient medicine, and especially of that of Rome. He gathered in and reduced to order the whole range of medical science from the founding of the school of Coos to Hippocrates, from Hippocrates to the Greek schools of Alexandria, and from these to that of Rome.

I leave to the physicians the labor of illustrating the Galenic doctrines, their value, their errors, and of showing how much he

benefitted science and art. I shall merely say that when Galen treated of surgical diseases, because he was the most learned among the few anatomists of his time, he did so with precision, exactness, and an admirably clear perception of all the subjects on which he discoursed. He gave wise instructions on phlegmous, bandages, and the method of stopping arterial hemorrhages by compression, by double ligature and torsion of the cut vessel, as well as on the service of blood-clot in arresting the flow. He treated at length of false aneurism, of tumors, and of diseases of the eyes. He performed exsection of part of a rib, or of the whole of it, and of a portion of the sternum. As to the rest, it may be said he did not, in his surgical teachings, advance the art to that point to which his genius, his studies and his large experience of fifty years in Rome should have led us to hope for."

The author then tells us that after Galen, medicine kept pace in degradation with the other sciences and arts, and continued involved in gloom and that mysticism which has ever been the handmaid of imposture, until the restoration of Greek literature and the invention of the art of printing broke through the all-pervading darkness, and mankind once more began to give free rein to those inherent mental powers which had for so many ages been held in brutal bondage. The following paragraph from the close of Burci's first period of the history of surgery, presents a sad picture of the debasement into which the art had fallen in the beginning of the sixth century:

"But what was the fate of surgery when the barbarians invaded the great part of Southern Europe, and under the standard of the blindest ignorance destroyed monuments, libraries, arts, sciences, letters—everything? Surgery, which had never formally been separated from medicine, passed essentially into the hands of ecclesiastics; every cloister had its physicians, and also its school, and then came forth mercenary and ignorant empiries, such as were the Pereodexti in the times of the Asiatic and European Greeks, who, dividing the work, undertook, some to cure diseases of the eyes, some fractures and dislocations of the bones, some wounds, etc. But where had these studied? Who had given them authority to practice? The practice of the family came down in the family; it was undisturbed by governmental oversight, and became a mere trade, exercised as many others, without either conscience or charity. Malgaigne, in his classical introduction to the works of Ambrose Paré, relates, and De Renzi

confirms the statement, that in the Codex of the bisigottes of the year 504, in which the practice of medicine is regulated—certainly then more followed by the free than by the claustral class—that should a physician have at any time bled a patient, and have taken too large a quantity from one of free condition, he must forfeit 100 *sous* (soldi) if the person survived, and in the case of death, he must be delivered up to the relatives, who might do with him whatever they pleased. If the patient was a slave, he must restore for him one of the same value. When a surgeon or physician was called to a sick person, he must fix the amount of his fee and receive earnest before undertaking the case, and in case of death he had no right to any payment. *The operation for cataract was rated at five soldi* (five cents), *provided the cure was complete.*

Clinical Reports from Private Practice.

A Case of Incomplete Strangulated Hernia, Reduced After Eight Days.

By W. B. Craig, M. D., Prof. of Anatomy, St. Joseph Hospital Medical College, of St. Joseph, Mo.

Upon the night of December 22d, 1879, I was called by Mr S., a medical student, who requested me to visit his brother-in-law, who was suffering from some obscure trouble of the bowels. At first I hesitated, but regarding it as an imperative duty, consented to go and do what I could, although the patient was reported to be in a dying condition. On my way to the patient's house, I gathered the following history of the case. It seems that Mr L., the patient, had a hernia once before, which was reduced with some difficulty, and that eight days previous to my being called, he had, while doing some heavy lifting, been taken suddenly ill, attacked with obstinate vomiting, constipation, and intense pain in the lower portion of the abdomen, the vomiting, in a short time, becoming slightly stercoraceous. Upon his arrival in town, in the afternoon of the day he received the injury, the family physician called to see him, and prescribed for him. This condition of affairs had existed until the evening I was called.

On arriving at the house, found the patient natural and in

possession of the faculty of speech, which singularly had been lost at different times during his confinement to bed. He was suffering excruciating pain over the abdomen, which was increased on pressure; lower extremities flexed on pelvis; abdomen very tympanittic. These symptoms pointed to peritonitis; casual inspection revealed nothing abnormal or unusual; however, upon a more careful examination, the fold of the right groin seemed a shade fuller than the left. Palpation elicited a slight doughy feel, but no tumefaction or swelling or great tenderness over the inguinal region, the cord alone being sensitive. Careful examination at this time failed to reveal any hernia, my efforts being directed to the reduction of an oblique, if any existed. Retiring to the next room, I gave a grave prognosis and suggested the propriety of temporizing until morning and then of an operation. Altnough I could not determine positively the presence of a hernia, yet strongly suspected one near the internal ring. Again, as it had been over a week from the date of the attack, the strangulation could not of course be complete, and a short delay could not endanger him materially. In the meantime I could prepare assistants, etc., and have good light. However, upon still further deliberation, I decided to again attempt to render a clearer diagnosis. I elevated the lower extremities of the patient, and thus by gravitation aid my efforts still further in reduction of any small loop of gut that was imprisoned. I varied the manipulations somewhat from those of the first examination, making direct pressure backwards as in returning a direct hernia. Fortunately my efforts were at last crowned with success; the small loop of intestine or omentum, or possibly both, could be felt gliding rapedly by the conjoined tendon, which was tense and hard under the finger. Before I could say a word the patient cried aloud, "There, its in," and the diagnosis additionally confirmed by an instant return of vomiting. Nothing of interest transpired after this, the usual routine treatment being adopted. Upon the second day, my patient was on his feet and has visited my office frequently since and returned to work.

This was undoubedly a very obscure case, from the small size of the loop caught by the conjoined tendon, the lack of the usual local symptoms of strangulation; and yet it undoubtedly was, as the treatment more than proved. The yielding nature of the canal permitted the direct protrusion, which passed a little obliquely to escape at the external ring, and was there caught as it passed around the conjoined tendon of the internal oblique and transversalis muscles; but not sufficiently to completely arrest its circulation, which would have produced gangrene in twenty-four hours. As it was, however, another half-day would have proved fatal.

Proceedings of Medical Societies.

AMERICAN LARYNGOLOGICAL ASSOCIATION.

Bucco-Pharyngeal Tuberculosis or Phthisical Ulceration of the Fauces. By F. H. Bosworth, M. D., of New York.

[Reported for the Journal.]

I was called, on the 2d of April, 1879, to Andover, New Jersey, to see, in consultation with Dr. John Miller, of that place, a patient presenting the following history:

Mrs. C. F. C., æt. 21, married. Her mother and sister died of consumption; her father and three brothers are living and in robust health. She had always been perfectly well and had never been subject to any cough, catarrh or throat trouble.

On February 8th, Dr. Miller told me, he had been called to see her and found her suffering from an attack of acute follicular pharyngitis, involving the soft palate and pillar of the fauces. There was deep submucous infiltration, the uvula being greatly swollen; fever mild, pain slight, deglutition quite painful. She was able to be up, however, and assist in the duties of the household. The Doctor saw her once in four or five days, and under treatment the disease gradually subsided, and by the first of March she was convalescent.

On the evening of March 6th she was married, though at the time she was somewhat delicate, without being able to refer her symptoms to anything more than the slight discomfort which arose from the condition of the throat. On the day following her marriage, March 7th, she was taken with a chill. On the 8th Dr. Miller saw her again, when he found her suffering from an attack of acute laryngitis, with violent fever, high temperature and rapid pulse. There was now considerable pain referrable to the fauces and larynx. Deglutition was extremely painful, with slight, hacking cough. The site of the original follicular inflammation of the fauces became now the seat of apthous patches, extending over the soft palate, uvula, the wall of the pharynx and the palatine arches. The condition in the larynx was diagnosed by the subjective symptoms, no laryngoscopic ex

amination being made. There was extreme prostration and the patient was confined to her bed. An examination of the lungs failed to detect any morbid condition.

From March 8th till April 2d, the above symptoms continued; there was considerable discharge of slimy, muco-pus, and the cough became more irritating and persistent; the fever remained high, the pulse rapid and weak. Liquid food was taken in fair quantity—the swallowing of solid food became so painful as to be rarely attempted. The voice was not at any time markedly impaired.

On April 2d I saw her, about eight weeks after the first symptom of throat trouble was manifest, and four weeks after the laryngeal disease and the grave faucial symptoms had set in.

I first saw her on the evening of April 2d. I found her in bed, apparently fairly well nourished and not much emaciated, but presenting that peculiar facies which showed her to be suffering under some marked dyscrasia. There was a peculiar dirty-looking pallor about the face and lips, while at the same time there was the bright red hectic spot on each cheek; the skin was hot to the touch and very dry; the tongue coated and parched; breath hot, feverish and rapid; axillary temperature 105°; pulse 120. An examination of the lungs showed dulness and broncho-vesicular respiration, verging on bronchial in the right inter-scapula region, but no râles in any portion of the lungs.

On examining the fauces with the tongue depressed, I at first simply saw the parts showing a uniform pallor throughout the whole region, and covered with a thick, tenacious, slimy, muco-pus; but on second and closer inspection, that there was ulcerative action going on, and involving the whole posterior wall of the pharynx, the soft palate and uvula on the right side, extending to the hard palate, the palatine arches, and a portion of the soft palate on the left side. On the right side the palate was destroyed as far as the glosso-palatine arch, but so evenly and smoothly as to almost escape notice, save from the lack of symmetry on the two sides.

An examination of the larynx showed the epiglottis, ary-epiglottic folds, arytenoids and commissure thickened and involved in that peculiar form of ulcerative action which we all recognize as advanced laryngeal phthisis, the ulceration involving the false cords, but the true cords were intact.

The ulceration in the pharynx was peculiar and characteristic, and I could not but pronounce it phthisical.

The closest examination and comparison of the laryngeal and pharyngeal disease failed to show any difference whatever, on inspection, between the two. There was superficial waste or destruction of tissues; there was an almost total absence of any evidence of granulation tissue or attempt at repair; there was no well-marked line of demarcation; there was no depression of the edges, and the depression of the ulcerated surface shaded off to the edges, which were somewhat ragged and irregular; there was no areola of inflamed membrane beyond the ulcer, and it was of a grayish color; but so also was the mucous membrane of the whole fauces, and the general coloration was not markedly different in the healthy and diseased portions. The diseased surface was coated with a slimy, ropy mucus, which I believe is peculiar to phthisical ulceration. We generally speak of it as muco-pus; the pus cells are probably comparatively few; it is mainly mucus and the debris of the wasting process covering and adhering to the ulceration, and partially concealing or masking it, and is removed and voided with considerable difficulty. Frænkel likens the surface of a phthisical ulceration to cut bacon; Laboulbene to the track of an earth worm over moist land. It is difficult to accurately describe it, but when once seen, it is unmistakable.

The subsequent history of the case was simply that of futile attempts to arrest the disease and partially successful attempts to relieve. She died April 15. The lung symptoms developed somewhat, but not markedly. Typhoid symptoms set in, with diarrhœa, on April 8, and she died of exhaustion on the 15th. The high temperature persisted to the last, rarely being reduced below 103°, though quinine was given freely.

The case was one of extreme interest to me, and although I saw her but once, Dr. Miller kept me fully informed concerning the progress of the disease. The main interest, of course, was in establishing in my own mind what had been before only a shadowy belief, viz., the possibility of phthisical ulcerations occurring in the pharynx; for while the reports of such cases had come under my notice, they had always been invested with a degree of uncertainty as to the correctness of the observations.

Medical literature, as far back as Hippocrates, has contained

vague references to symptoms of pulmonary consumption which might refer to faucial ulceration, but the first definite report of true phthisical ulceration in the pharynx I find in the report of St. Bartholomew Hospital for 1871, by Sam'l Gee, who reports the autopsies made in two cases in which the disease terminated fatally after a course respectively of four and six weeks, and in which there was found miliary tubercles in the lungs, liver, spleen and other organs, and scattered nodules in the base of the faucial ulcerations. In the St. Bartholomew Hospital report for 1875, the same writer reports a third case in which the pharyngeal disease set in upon general and pulmonary tuberculosis, terminating fatally after six months, the autopsy again revealing tubercles in the lungs, liver and spleen, intestines, and also scattered nodules around the base of the ulcerated surface in the fauces. Dr. Gee prefaces the report of the third case with some historical references and with the assertion, that although no other cases had been reported, these cases entitled phthisical ulceration in the pharynx to a recognized position in the category of diseases.

Since 1875 a number of cases have been reported by Cornil E. Wagner, Isambert, Frænkel and Laboulbene; and Frænkel has accompanied his report with such a complete history and description of the disease, that it leaves little to be said and I will not occupy your time by any unnecessary recapitulation, but will very briefly generalize.

During the course of general or pulmonary tuberculosis, but more frequently as a primary manifestation of a general dyscrasia or diathesis, there sets in upon the pharynx, soft palate or neighboring parts, an ulcerative process, presenting the appearance described in the case above reported, and which we call phthisical or tuberculosus ulceration; there is severe pain always and often of a most acute and lancinating character. The discharge is not purulent but a dirty, grayish muco-pus.

There is, as a rule, high fever from the onset, the temperature varying from 103° to 106°—a persistent and continued fever and not controlable by quinine—this is my own observation and it is confirmed by other reporters, especially Frænkel, who reports the total inefficiency of the drug to reduce the temperature. The tendency of the ulceration is to extend laterally, and also very soon to the larynx, though I find no report of any case in which laryngeal disease has extended to the fauces.

The course of the disease is rapid; this is especially the case if the pharyngeal ulceration is primary; in these cases death ensues from exhaustion in from four to eight weeks. If the pharyngeal disease is secondary to pulmonary tuberculosis the fatal termination may be postponed from four to six months.

The diagnosis is quite simple to one familiar with the laryngoscopic appearance in laryngeal phthisis, yet so accurate an observer as Frænkel made the mistake of placing several of his patients, suffering from this disease, under anti-syphilitic treatment, not only failing to benefit them thereby, but doing them absolute injury. The only other disease with which it may be confounded is scrofulous ulceration, but grouping the prominent symptoms of the three forms of ulceration together, we will find the differences well marked, as follows:

SYPHILITIC ULCERATION.	PHTHISICAL.	STRUMOUS.
Deeply excavated.	No apparent excavation.	Moderately excavated.
Bright red, angry looking areola.	No areola.	Moderately discolored areola.
Sharp cut edges.	No well defined or sharp cut edges.	Slightly everted edges.
Bright yellow, purulent discharge.	Grayish, thick, semi-opaque mucus disch'g.	Muco-purulent discharge.
Free and copious disch'ge	Small in amount.	Small in amount.
Rapidly destructive.	Erodes slowly.	Erodes slowly.
Extends deeply.	Extends laterally.	Extends laterally.
No general dyscrasia.	Marked constitutional disturbance.	General debility.
No fever.	High fever.	No fever.

We come now to the question, what is the disease and how far is tubercle concerned in its production? I do not propose to enter into a discussion of the pathology or quote authorities. In a paper on laryngeal phthisis, read before the County Medical Society in this city, in March last, I took the ground that laryngeal phthisis is not caused primarily by a deposit of tubercle, but that we have, under the influence of a marked dyscrasia, an attack of acute follicular inflammation, failing to resolve, and that the contents of the inflamed follicles degenerate, break down and develop an ulcerative process, and that the few so-called tubercles that are found about the base of the ulceration are a secondary deposit. This conclusion was based mainly on clinical observation, and was held to more fully harmonize with the development and course of the disease than any other explanation.

Now, applying this explanation to the case reported above:

This young lady, while apparently in good health, caught cold, and it manifested itself in an attack of acute follicular inflammation of the mucous membrane of the soft palate; under simple remedies the disease was subsiding, when there was suddenly manifested in her general system that profound general dyscrasia which had carried off her mother and two sisters and which was a perpetual menace to her during her whole life, viz., the tubercular diathesis. Under its influence the inflamed follicles which were undergoing resolution, now took on ulcerative action and the typical waste set in which characterizes the disease. There was now manifest a very grave general condition, and yet no local manifestation of it save an ulcerative process of moderate extent in the fauces. The general fever and accompanying symptoms were something more than a symptom of the local disease. It was a systemic condition which arrested the process of resolution which was going on, destroyed the *vis mendicatrix naturæ*, and so far dominated the reparative processes going on as to utterly change their character and substitute in their place a destructive process.

The local disease and the blood condition progressing, the patient finally succumbs and dies of exhaustion in acute miliary tuberculosis; and probably had an autopsy been made, we should have found, as were found in most of the cases reported of phthisical ulceration of the pharynx, miliary tubercles in the lungs, liver, spleen, intestines, but in the pharynx, ulcerative destruction of tissue and a few scattered gray nodules about the base of the diseased surface.

DISCUSSION.

Dr. Beverley Robinson — The case just reported by Dr. Bosworth interests me particularly. I cannot say I am of opinion that miliary tubercles would probably have been found in the larynx of Dr. Bosworth's patient. I have had occasion to observe several cases of generalized acute miliary tuberculosis within the past few years. Of these cases, three or four were carefully examined as to their intra-laryngeal condition prior to death. The gross appearances of the vocal organs, both during life and at the autopsy, were those of typical ulcerative phthisical laryngitis. Three of these larynges were presented by me to the members of the New York Pathological Society, and referred by them to the Microscopical Committee for examination. The re-

port of this committee was to the effect that *in no instance* was miliary tubercle found in these organs. And this fact was the more remarkable because miliary tubercles were found in large numbers, generally disseminated throughout the thoracic and abdominal viscera, at the same time. I have specimens in my possession, taken from a patient who died in my service at Charity Hospital only a short time since of generalized miliary tuberculosis. The lungs, spleen, liver, kidneys, intestines, etc., all contained miliary tubercules in larger or smaller numbers. The larynx is deeply ulcerated, and shows a considerable amount of œdematous swelling of the arytenoid cartilages and ary-epiglottic folds. Still, I do not believe, after close ocular inspection, that any miliary tubercles will be found by further research. I shall, however, endeavor to have it referred to the Microscopical Committee of the New York Pathological Society, and it will doubtless be reported upon fully and definitely. Perhaps I may be permitted to add that individually I have come to regard the presence of miliary tubercle in the larynx as very improbable. I do not affirm positively that it never exists there, but I believe I am justified in asserting that the larynx is a most infrequent location for its deposit. As will be clearly evident to all members of this Association, there is involved in this question one of great practical importance, viz., whether so-called laryngeal phthisis is curable or not. I, for one—and in that I agree with Dr. Bosworth—hold that it is. Can it be done by mere topical applications, without operative interference? Of this I am not sure, but I am willing to become convinced.

Dr. E. Cutter, of Boston—What is scrofula? I have been accustomed to regard it as a state caused by tuberculosis or syphilis or possibly cancer; thus when we speak of a scrofulous taint, we mean, generally, both syphilis and tubercle.

Now in the differential diagnosis so ably drawn by Dr. Bosworth, I did not note a diagnostic sign between syphilis and tuberculosis that I have used for ten years, and with gratifying results, practically. It consists in the morphology of the blood. Both states present enlarged white corpuscles, mycelial filaments and spores; but in syphilis the mycelial filaments and spores are copper colored, and the red corpuscles well rounded out, nummulated, and high colored generally. In consumption, on the other hand, the filaments and spores are not copper colored but

white, the red corpuscles are aggregated in confused masses and the hæmatic interspaces are filled with fibrine filaments. I do not say that I have made no mistakes, but I do say that I have cleared up cases by this method of differential diagnosis, inexplicable by ordinary means. My following is that of Dr. Salisbury, of Cleveland, Ohio. I am well aware that these views have not been largely received, still this has not interfered with my making use, and very satisfactory use, of the plan. I should be glad to have the Association enjoy the same advantage that I have.

DR. E. L. SHURLY — I have been very much interested in the Doctor's paper just given, and, in view of the paucity of literature on the subject, I consider his observations of particular value.

I have had in my practice two cases of this sort which I confess were very puzzling both as to exact diagnosis and treatment. In one of them, on account of the recurrence of spots of ulceration of the pharynx, I maintained for some time the conviction that syphilis was at the bottom of the trouble, notwithstanding no such history could be elicited. Soon, however, all doubt was cleared away by characteristic phenomena of pulmonary tuberculosis rapidly supervening. I consider it noteworthy, to say the least, that the pharyngeal implication was so decidedly manifested in advance of the pulmonary signs.

The other case presented, for a long time, unusual swelling, with paleness of the pharyngeal follicles, in addition to clubbing of the arytenoid processes. But no notable implication of either the larynx or the lungs was manifested until the glands of the pharynx presented marks of advanced disease.

As to the question of arresting the progress of disease by any method of local treatment, I have nothing to add.

Dr. F. I. Knight, of Boston, said that he had seen several cases in which there was deep ulceration of the pharynx in connection with pulmonary disease, but had seen only one in which the disease seemed primary in the fauces, though he had met with several in which the larynx seemed to be primarily affected. The case referred to was that of a gentlemen who complained at first only of dysphagia. On examination of the fauces, the arches of the palate, especially on the left side, were seen to be studded with small granulations, which subsequently ulcerated and be-

came more or less confluent. Later physical signs of pulmonary disease were detected at the apex of the left lung. The case terminated fatally in a few months. Dr. Knight said that according to his experience one rarely meets with such an appearance in the pharynx or larynx as would lead one to suspect a deposit of miliary tubercle, though from some of the text books one might infer that such a condition was not uncommon.

TRI-STATE MEDICAL SOCIETY.

LACERATION OF THE CERVIX UTERI—THE HISTORY AND DIAGNOSIS. By E. C. DUDLEY, M. D., Former House Surgeon. to the Woman's Hospital in the State of New York.

The value of the operation for closure of the lacerated cervix will be generally admitted, but widely differing opinions exist; first, respecting the frequency of the lesion; second, respecting its extent when it does occur, and third, respecting its relations to the various erosions and so-called ulcerations of the cervix, of which it is held that this lesion is an important cause. January, 1879, in a paper* before the Chicago Gynæcological Society on this subject, I considered the history, diagnosis and treatment; but notwithstanding this and other contributions to the same subject, some of them by eminent gynæcologists, I am convinced that the history and diagnosis are even now not well appreciated by many. I shall, therefore, make use of the parts of the paper just mentioned, and of some additional notes made since its publication, together with two drawings, for the purpose of explaining more clearly.

CLINICAL HISTORY—During labor there must be some point in the cervix above which all the tissues of the uterus contract for the expulsion of the child, and below which they dilate for its exit. "After delivery examination shows, at a point above the normal site of the external os, about on a level with or a little below the plane of utera-vaginal junction, the familiar,

* Published in the Chicago *Medical Journal and Examiner*, March, 1879. The quotations which follow may be accredited to this paper.

hard, unyielding ring vaguely called the os." This cannot be the internal os, since its location is at or below the vaginal junction, while the internal os is considerably above. "Neither can it be the external os, since its location is too high; it must, therefore, be between the two, at the very point just mentioned, above which contraction, and below which dilatation occur in labor. It is, therefore, a temporary intracervical os, below which we must look for all that part of the cervix which during labor was compelled to undergo excessive dilatation, and expect there to find laceration, if it be present. Now without great care this portion of the cervix, which had been so stretched that it cannot immediately recover its contractile power, will be entirely overlooked, but it is always to be felt projecting into the vagina, to the touch not unlike a section of large intestine, and as destitute of contractile power as the sphincter ani muscle after extreme forcible dilatation by the method of Van Buren. It is usually one or two inches in depth, and so deceptive to the touch that the attempt has often been made to remove it by the finger for a clot of blood. After normal labor this slowly recovers its contractile power, and in a few days resumes its normal shape, and the external os is thereby restored." But if laceration occur nature has all the conditions for concealing the evidence of the lesion so artfully that to ocular examination the only signs which remain to mark its presence are not uncommonly congestion, enlargement, and the so-called ulcer or erosion of the cervix. The lacerated "diverging flaps," as Emmet says, "are at once forced in the directions offering least resistance, the posterior catching on the recto-vaginal wall and crowding backward into the vaginal cul-de-sac and the anterior forward in the axis of the vagina. The congested tissues about the temporary os, which we have called intracervical, meeting no resistance, now roll out, and this eversion, by obstruction to the circulation, causes the tissues to become so gorged with blood that they no longer have sufficient space for their accommodation within the uterus, and the eversion continues until tissue enough for the formation of a spurious cervix has been rolled out in the vagina, and until the temporary intracervical os has actually usurped the place of the normal os externum which had been destroyed."

The above figures not only show the manner in which the false cervix is formed, but they explain that its immense size is

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in a large degree only apparent. Observe that the true vaginal attachment is at X, Z, the true vaginal portion of the cervix therefore must be below these points, but by the reduplication of the vaginal tissue it appears to extend to the apparent attachment X′, Z′, so that all below X′, Z′ appears as the vaginal portion of the cervix.

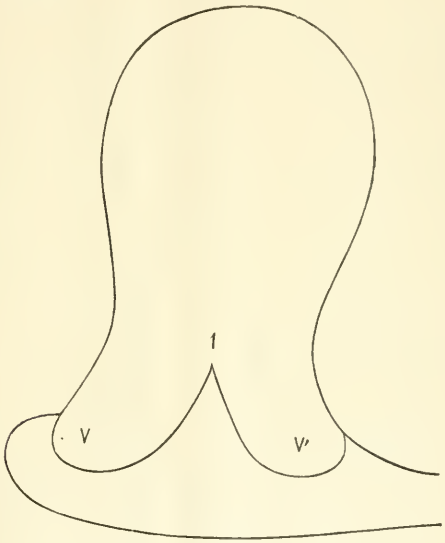

FIGURE 7—V and V′ represent the margins of a recently lacerated cervix. V, the posterior lip, is crowding backwards into the posterior cul-de-sac; V, the anterior lip, forward in the axis of the vagina. 1 represents the site of the temporary intracervical os at the bottom or angle of the laceration.

This reduplication of the vaginal tissues from X, Z to X′, Z′ (Fig. 8) also accounts for the difference of opinion so common at the present day respecting the extent of these lacerations. I frequently meet with cases which have been diagnosed as very slight lacerations not involving the utero-vaginal attachment, but upon examination find that they extend sometimes an inch or more past this attachment into the vaginal tissues on each side. This very common deception is explained by the fact that the reduplicated vaginal tissues may, as indicated in figure 2, extend farther than the laceration, so that however great the laceration may be it does not extend past the apparent vaginal attachment. This deception may be exposed by placing the

patient in the knee-chest position, when the uterus will be carried by its own weight toward the diaphragm and the reduplicated vagina will unfold, and the laceration which appeared not to involve the vagina will be seen often to extend a considerable distance into the vagina on both sides.

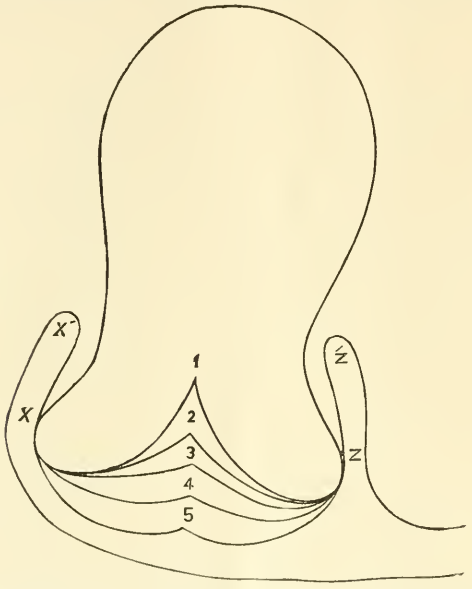

FIGURE 8.—This figure represents the false cervix composed of reflected vaginal tissue and outrolled or everted intrauterine tissue; this outrolling has taken place as represented by the curved lines forming the angles 1, 2, 3, 4 and 5. These lines indicate the gradual process of eversion. The intracervical os, point 1, has been rolled out to point 5, where it appears to be the external os, the vaginal attachment, points X and Z, now appear by a reduplication to be at points X′ and Z′, so that the cervix appears longer and larger than it actually is.

Immediately after the operation for closure of the lacerated cervix it is noticeable that the immense false cervix has been so much reduced that its size is often smaller than normal. Dr. Emmet, in his recent book, "The Principles and Practice of Gynæcology," declares that he has been unable to account in full for this decrease. He explains it in part by the rolling in of the everted tissue, by the relief from congestion due to hemorrhage, by the restoration of the normal circulation due to the readjustment of the flaps, by the loss of tissue in denuding

and by the rupture and emptying of the cysts of the mucous follicles so often present. There is, however, an additional and more important reason for this decrease, which in connection with the foregoing may complete the explanation. It is that the reduplication of the vagina is entirely disposed of by the operation, so that the true vaginal attachment is made to appear in its true relation, and so that the apparent attachment, which had contributed so much to the formation of the false cervix, is destroyed. A thorough understanding of this reduplication I am satisfied would clear up much of the dispute respecting the frequency and extent of this lesion.

The delicate intracervical membrane accustomed to the mild alkaline secretion of the uterus is now in contact with the irritating acid secretion of the vagina; the uterus, heavy from congestion, now falls to a lower plane in the pelvis, and is exposed to friction and to other mechanical injury; the formation of the so-called ulcer or erosion of the cervix is therefore inevitable, yet the cervix presents no evidence of laceration except the congestion, the enlargement and the so-called ulcer, because the diverging flaps are so modified that all indication of their previous contact has been obliterated.

DIAGNOSIS—This lesion was formerly known by its effects, and accordingly various names were applied to designate their extent and character: "erosion," "granular erosion," "papillary erosion," "ulcer," "follicular ulcer," "cockscomb granulations."

"There is a form of erosion solely dependent upon an irritating discharge from the endometrium or vagina, apt to occur in feeble and badly nourished subjects, and not very uncommon in virgins: a condition analogous to the familiar erosion and excoriation produced by prolonged nasal discharges on the upper lips of children. Such an erosion is readily distinguished from that of laceration by the absence of eversion, absence of marked cervical enlargement, by the presence of a normally shaped os externum, and by physical examination soon to be described. It may be cured by removing the cause. It is generally associated with laceration, and when associated its treatment should be insisted upon as preparatory to the operation."

But when the erosion or so-called ulcer appears on the enlarged and everted cervix, it is always dependent upon laceration. The word fissure ordinarily employed to designate laceration is misleading, and its introduction in this relation was, therefore,

unfortunate, since in the majority of cases the fissure which we should naturally expect to find on the lacerated cervix has been obliterated by the everted intrauterine tissue, which we have said forms a great part of the false cervix. If, therefore, we regard fissure as essential to the diagnosis we shall generally fail to detect the worst cases. Something more than digital and ocular examination, therefore, are required to demonstrate the presence of this everted false cervix. The most reliable means of diagnosis are the two uterine tenacula and the Sims speculum.

The tenacula are absolutely essential, and I am fully convinced that their importance is not appreciated by many who, neglecting their use, not only fail in diagnosis, but question the accuracy of others who succeed. With these instruments, Emmet first untied the knot and revolutionized almost the entire pathology and treatment of cervical disease. The late Dr. Peaslee, in his remarks on this subject before the N. Y. Co. Med. Soc., referring to the large number of cases of so-called ulceration, says 'that they were not recognized, for none of us knew anything about them till Dr. Emmet told us. It was he who, in a happy moment, brought the anterior and posterior surfaces together with tenacula, and instantly demonstrated that what we all supposed an ulceration was nothing more or less than a laceration.'"

The following is quoted from the history of Dr. Emmet's first case, November, 1862. A typical case of so-called "ulceration." "The uterus was some four inches in depth, and an erosion extended about two inches in diameter over an enormous cervix. With great care this erosion had been healed several times, by maintaining the recumbent position for a sufficient length of time, but a relapse to the former condition recurred in each instance shortly after beginning to exercise by walking. I had almost despaired of being able to offer her any permanent relief, and attributed my want of success to the condition of her general health. While making a digital examination one day, I was puzzled to account for the greater width of the cervix in comparison to that of the body beyond, a condition I had for the first time appreciated. I placed her on her left side, and with Sims' speculum brought the cervix in view. I drew the posterior lips forward toward me with a tenaculum, but with no special purpose, when I was surprised to observe that it had decreased to nearly half its previous size. On lifting up the anterior lip with a tenaculum, in the other hand, so as to bring

the two portions in approximation, the outline of a cervix presented, of nearly a normal size. The difficulty was at once apparent, for the parts had rolled back within the uterine canal, and a deep lateral fissure became evident, which extended on each side entirely through the cervix and beyond the vaginal junction. On separating the flaps and forcing them back to their former position, I saw the tissues gradually roll out, and the cervix again present its previous appearance. There could then be detected no appearance of laceration, and with the reduplication of vaginal tissue over the sides of the uterus, as I have already described, the cervix presented a normal length above its apparent junction with the vagina. The remedy at once suggested itself; the operation was performed with the aid of my assistant."

The Sims speculum is also very important, as the ordinary bivalve puts the everted tissues on the stretch and prevents the approximation by the tenacula, and the ordinary cylindrical speculum does not afford sufficient room.

But the conveniences for using the tenacula and the Sims speculum are not at the disposal of every hurried practitioner, and the desirability, therefore, of a more ready means of diagnosis is manifest. Fortunately we have the means, and they have already been indicated. Simply translate the words ulceration or erosion with enlarged and everted cervix (*mark you the enlargment and eversion must be present*) into laceration of the cervix, and the diagnosis will be correct in every case. In this false cervix we have an explanation of the cause of many cases of subinvolution. I have repeatedly observed rapid and entire relief from the evils attendant upon subinvolution to follow the operation for the closure of the cervix. As already explained, this enlargement and eversion of the cervix was formerly supposed to be the result of hypertrophy, but since it has been demonstrated by the uterine tenacula that this everted tissue may be rolled back within the uterus whence it came, and that the cervix may thus be made to assume its normal size, hypertrophy of the cervix is rarely encountered. The word as applied to the cervix has almost disappeared from my case books.

Although the province of this paper is more specially the history and diagnosis, I shall, in closing, allude briefly to Emmet's operation versus the old methods of treating so-called ulceration with enlargement. The unsatisfactory results of the former

methods of long continued topical applications to the uterus are proverbial. Patients have been passed on from one doctor to another only to undergo the same routine exposure and application once or twice a week, sometimes improving, but often relapsing again into the same old condition of chronic "ulceration." The moral influence on the patient of all the exposure which long continued local treatment necessitates, cannot be other than pernicious, if indeed this moral influence be not productive of more evil than the disease itself. Fortunately we have a mild,* prompt and efficient substitute for much of this local treatment in the operation for closure of the lacerated cervix, to which the name Trachelorrhaphy has been given. Its indication is the presence of that which in former times would have been diagnosed ulceration of the womb, with enlargement of the cervix; in other words, the presence of the false cervix The operation is of course liable to abuse. It may be performed contrary to the indications, but very soon it will take its place among the most satisfactory of modern surgical procedures. It is my opinion that if this operation were followed by none of the good results so confidently claimed it would be a justifiable procedure, inasmuch as it causes those tissues which have been the object of so much violent and fruitless treatment to be removed from the field of the speculum and from the approach of the porte caustique.

*At the Woman's Hospital, Emmett and others almost invariably use ether in this operation, but is generally unnecessary. Of the fifteen operations in my own practice since July, 1879, anæsthesia has been dispensed with in twelve, and the patients did not complain of the pain from the operation as much as of the cramped position and the pressure of the speculum.

ST. LOUIS MEDICAL SOCIETY.

[Reported for the Journal.]

Central Lesions in Neuralgia.

Dr. C. H. Hughes—You will recollect, Mr. Chairman, the other night, when the discussion on neuralgia was up, I was challenged to produce authorities, and I promised to produce them for the statement I made in regard to the existence of central lesions in neuralgia. I have in my hand Rosenthal on Diseases of the Nervous System, and a translation of Velpeau's Introductory Lecture on Diseases of the Nervous System. "In dealing," says Rosenthal, "with neuralgia, the physician should, above all, determine both with regard to prognosis and treatment, whether it is merely the expression of a local irritation or the peripheral indication of central morbid process." I need not quote him further. Let me read briefly what Velpeau says: "We have sought especially to show that in a number of cases neuralgia is of central origin, and even in the cases where the cause of neuralgia is evidently peripheral, there is, at the end of a certain time; participation of the nerve centers. This participation explains the extension of neuralgia frhm the primarily affected nerve branch to the branches of the same trunk, and what is more frequent, to another nerve trunk. Secondly, the persistence or relapse of neuralgia in certain cases of section of the nerves along the tract, which appeared to be a locality of the pain. Apropos of these questions, we should discuss new theories based upon more or less precise knowledge of the peripheral branches; theories put forward to explain the persistence of neuralgia, after division of nerves, and their extension to other branches. In the presence of these two theories—one explaining all by central nerves of communication, and the other by the peripheral—we take sides with the former, not entirely disputing a certain part played by peripheral anastomosis. Nerve actions we have seen cause lesions, not only in the peripheral, but even in the central nerve ends. Experiments made by various writers have demonstrated that irritative nerve lesions cause sometimes myelitis, with paralysis. Nerve-stretching in a rabbit, a cat, and even simple division or cauterization have given rise to more or

less extensive myelitis. I have recalled to you the importance of these facts in explanation of so-called reflex paralysis and reflex atrophies, etc." He gives an example in one of his patients who had been struck in the lower part of the right leg by a piece of shell, who had not improved in two years; atrophy set in at the end of that time, finally involving the whole limb. Concerning this, he says: "What has taken place? There has been a change in one of the nerves, including the wound; secondly, a change in the medulla spinalis; and finally, muscular atrophy." He goes on to say: "The mechanism of reflex atrophy is very well illustrated in Guinea pigs, between the sciatic nerves; they have been divided only through the largest part of that nerve. In these cases the small sciatic nerve, which had not been touched during the operation, after a certain time atrophies, and the muscles supplied by it atrophy also." The phenomena of traumatic tetanus is next instanced, which we alluded to the other night, likewise reflex epilepsia, convulsions, contractions, and reflex hysteria, as explainable from this secondary irritation of the spinal cord. Not only Velpeau, but all authorities, coincide with him, and I was surprised to hear any one gainsay the possible persistence of central lesion from peripheral irritation. What I have read serves to fully confirm the statement made on the previous evening, that nerve-stretching should be undertaken with great caution.

A Unique Letter.

DR. GREGORY—I want to read a letter from a distinguished gentleman of Dr. Hughes' specialty. I wish to read it so that young anatomists may look the subject up and inform me how the particular thing which was claimed to have been attained in the particular case referred to in the letter was reached. Dr. Prewitt perhaps knows that some years ago he was consulted by a prominent young gentleman of this city for a neuralgic affection. It proved to be so annoying the patient went to one doctor after another.

He was my patient for a time. This young man, after trying a number of physicians, went East to consult some one there. Before starting, I advised division of the nerve that was implicated. He returned, claiming that the nerves had been divided by a distinguished gentleman. He would tell me that the cutting was done, but I was at a loss to know where the nerve was divided. The disease returned, and I requested him to write to

the physician, and find what it was that relieved him. In reply to a letter, he received the following answer:

"The nerves I cut in your case were the cutaneous branch of the eleventh and twelfth dorsal, and the first and second, third and fourth lumbar. I think it would serve you to have the operation repeated." Yours sincerely,

William A. Hammond.

I must say I am still puzzled about what was done, as I was before the letter was given me.

On the Relations of the Placenta to Postpartum Hemorrhage, by Walter Coles, M. D.

Mr. President:—In *postpartum hemorrhage* the general advice of obstetrical authorities is embraced in two injunctions; secure uterine contraction, and, if the placenta is still retained, see that it is promptly delivered. Now, since it is a universally admitted fact that a tonically contracted uterus cannot bleed, it must be conceded that the former of these aphorisms is a *sine qua non*. But the second, in regard to the removal of the placenta, admits of qualifications which we would do well to study.

First, let us inquire in what way the presence of the placenta within the uterus contributes to the hemorrhage or to its continuance. It is claimed that a retained placenta may produce hemorrhage for two reasons, (1) because it offers a mechanical obstacle to perfect uterine contraction; (2) because, if partially adherent, it directly promotes the bleeding. (When there is no detachment of the placenta there can, of course, be no hemorrhage.) It will be readily seen that two physical elements must necessarily enter into all cases of *postpartum* hemorrhage. There must be imperfect uterine contraction and partial or complete placental separation. As a rule, it will be found that the profuseness of the hemorrhage will be in direct ratio to the uterine inertia, and inversely proportional to the area of adherence of the placenta. That is to say, the flow of blood will be in direct proportion to the laxity of uterine fibre, and to the extent and number of ruptured vessels in the utero-placental site. If this be true, it is evident that in ordinary labor the presence of the placenta *in utero* can only promote hemorrhage in so far as it offers a mechanical obstacle to uterine contraction, whilst it really lessens it to the extent of its adherent surface. Although we are aware that this latter proposition is in direct conflict with high

authority, we think it tenable on physiological and clinical grounds, and hence we are justified in concluding that the only prossible good which can accrue from a speedy delivery of the after-birth in *postpartum* hemorrhage is the mere mechanical one of clearing the cavity of the womb of so much foreign material in the same manner, and for the same reason, that we would sweep out so much clotted blood, simply to get rid of it, and in order to clear the way for local styptics within the uterine cavity.

I desire to provoke a discussion of this question, because I am satisfied that there is much to be said on both sides of it, and for the reason also that most of our text books offer very loose and empirical advice on the subject, which is well calculated to mislead students and practitioners who are taught to regard the presence of the placenta in the uterus as a thing necessarily hurtful, and to be gotten away at all hazards and as speedily as possible. Entirely too much stress being laid upon the retention of the afterbirth and too little upon the fundamental fact that the placenta has, in nine cases out of ten, nothing whatever to do with the bleeding, which is rather due to absence of uterine contraction, to *inertia*. In proof of this assertion, it is only necessary to recall the fact that in ordinary labors the retention of the placenta, whether partially or wholly detached, does not produce hemorrhage. Common experience teaches us this, and so long as we are convinced that the uterus is firmly contracted upon the placental mass, we all are accustomed to feel satisfied and await with composure and confidence the advent of secondary pains, and when finally, after a rest of fifteen or twenty minutes, the placenta is thrown off, its expulsion is accompanied by no abnormal flow of blood. Yet it must be evident to any mind that if the placenta were the offending cause in these cases, every mother would bleed nearly to death before this body could be gotten rid of in the course of nature.

We repeat that most of our obstetrical works are not sufficiently discriminating and explicit on this point; they do not explain with clearness the mechanism of the hemorrhage, and the part which the retained placenta plays in promoting it, but leave the student to infer that its retention is really the foundation of all the trouble. The practice in these cases partakes too much of a blind, routine procedure. For instance, Leishman, in his chapter on "Hemorrhage After Delivery," reminds the student in the first paragraph that "Retention of the placenta and *consequent hemor-*

rhage may be the result of mismanagement." Undoubtedly this is true in some instances, but upon reading a general statement like this upon the very threshold of the subject, the inexperienced practitioner, who naturally looks to his books for advice, will reason thus: if the hemorrhage is a "*consequence*" of retention of the afterbirth, and such retention the result of a palpable "*mismanagement*," then, of course, the proper course is to empty the uterus as the first step towards eradicating the mischief. And he will probably proceed to do this even in the absence of uterine contraction, a measure, which in the latter case, while accomplishing no possible good, would probably make matters worse. Still, in every event, whether there be inertia or not, the student has the same dogmatical advice to guide him, for, says this author, on the next page, "In all these cases the treatment is the same, and consists in the speedy removal of the placental mass," (p. 391). Dr. Playfair, whose work is the best recently published, lays the usual stress upon the importance of *emptying the uterus*. He says: "When the placenta is retained it is the more essential, as the hemorrhage cannot possibly be checked as long as the uterus is distended by it." Now, we ask, is this proposition of Dr. Playfair's true? Is it a fact that the mere bulk of an ordinary placenta (not abnormally adherent) can sufficiently *distend* the uterus to provoke serious hemorrhage? A moment's reflection will soon convince anyone to the contrary. Indeed, I think that the author has elsewhere in his work, clearly answered this question in the negative. We have already called attention to the fact that when the uterus contracts properly there is, in common labor, no hemorrhage during the interval between the birth of the child and the natural extrusion of the placenta, a period covering from ten minutes to half an hour, and hence the conclusion is irresistible that so long as the womb performs its part faithfully, the mere mechanical presence of the placenta is incapable of harm. So true is this, that Dr. Playfair, in his chapter on the "Management of Natural Labor," takes particular care to warn his readers against "*undue haste*" in removing the afterbirth, a practice which he very properly believes "tends to increase the risk of postpartum hemorrhage (p. 279). He moreover adopts the rule laid down by McClintock, that fifteen or twenty minutes should elapse before making any attempt to deliver the afterbirth.

Before discussing this question further it would be well to

revert briefly to the mechanism of postpartum hemorrhage. What are the causes of hemorrhage and whence comes the blood? I answer that in flooding from the utero-placental site, the causes are various but the source of the blood is always the same. It is important to remember that dangerous and even fatal hemorrhage may occur postpartum, which is quite independent of the placenta, as in intra-uterine fibroids, laceration of the cervix, vagina, etc., but such being excluded from present consideration, leaves us to deal with but three classes of cases. The first of these, and by far the most common, is uterine *inertia*; the second is abnormally adherent placenta, in which case the placenta, through structural changes in the decidua serotina, becomes so firmly fastened to the uterine wall as to interfere with proper contraction of the organ. Here we may have a normal disposition on the part of the uterus to contract, but with an inability to do so, in consequence of the firm placental engraftment, the result being irregular and imperfect contraction. It is to this class of cases that what is known as hour-glass contraction usually belongs. There is also a third class of cases, due either to disease of the the uterine fibres, to old uterine adhesions from previous inflammation, or to deranged innervation in which there is likewise defective contraction. The source of the blood in all three of the classes of cases under consideration is, not from the separated surface of the placenta, but from the torn ends of uterine vessels, the arteries and veins, both of which, but chiefly the latter, contribute to swell the torrent. The well known views of Hamilton and Sir James Y. Simpson upon the mechanism of hemorrhage in partially detached placenta have created an impression upon the professional mind in keeping with the great weight of their authority, and I am satisfied that the writings, more particularly of the latter, have contributed largely to erroneous views of practice. It was contended by Dr. Simpson that in uterine flooding the hemorrhage was exclusively venous, and that the blood escaped, not from the denuded uterine wall, but from the venous radicles of the detached portion of placenta. Of course, if the view of Simpson be accepted, which is unfortunately vaguely done by many, there could possibly be no better alternative than to put in practice his operation, and at once separate the entire placenta and thus cut off the flow. But there can be no question that Dr. Simpson carried his theory too far, and that the theory itself was based upon a misinterpretation of

facts. Such is now the almost unanimous conclusion of all modern obstetrical authorities who have given the subject special attention.

Now, Mr. President, I do not desire to be understood as advocating anything like a revolution in the matter under discussion, but my object is to call attention to certain practical considerations based upon strictly physiological data, and which I think are calculated to promote more rational views of practice; for the fact is that no physician or surgeon can successfully cope with any grave emergency, unless he has clearly fixed, in his own mind at least, substantial reasons for whatever he may feel called upon to do. Empiricism often succeeds, but in the majority of cases, and always sooner or later, it leads to disaster.

In order to state more clearly the relationship of the placenta to the etiology and treatment of postpartum hemorrhage, I would present and defend the following propositions:

1st. The placenta can only participate in causing postpartum hemorrhage when it is either of abnormal size, or abnormally adherent—thus offering a physical bar to uterine contraction.

2d. Simple retention of the placenta when detached, or partially adherent by *normal* attachment, does not of itself contribute to the production of hemorrhage.

The first thing for the practitioner to do in a case of postpartum hemorrhage, is to examine the uterus externally and ascertain its condition as regards shape, size and rigidity. These interrogations can all be readily answered by an experienced hand, placed upon the abdomen of the patient. If the uterus appear firm, but oblong and somewhat irregular in shape, he is justified in concluding that there is something within it that offers a mechanical obstacle to perfect contraction and he knows from the feel that this obstruction must be either in the nature of a tumor, or an abnormally adherent placenta. Having satisfied himself that the uterus is willing to perform its part, he gently passes a hand within its cavity, in order to ascertain the nature of the hindrance, and if he finds the placenta partially adherent he endeavors to detach it. This is sometimes a difficult matter, and may fail of perfect accomplishment, even in the hands of the most skillful. But the completeness of contraction and the stoppage of bleeding will in this case

be in proportion to the amount of placenta detached, for when this has been pretty thoroughly done, the uterine fibres are left free to contract, and the uterus will generally close down upon the placenta and hand and extrude both into the vagina. Again, the attendant may find on external manipulation that the uterine body is much larger and not so firm as it should be; it is also rounded and more uniform in its outline than in the case previously described. Here we have a mixed case, where there is partial inertia together with distension from constantly increasing blood-clots. The distension in this case directly contributes to the bleeding, and should be relieved by passing in the hand and turning out the coagula. This should however be *preceded* and accompanied by the usual remedies for inertia. It is not necessary, in a case like this, to worry over the placenta, (unless abnormally adherent) until the uterine atony is overcome, in which event the uterus will take care of itself, whether the afterbirth is within it or not.

To my mind, abnormal adhesion, or abnormal size of the placenta, constitutes the only valid reason for its manual detachment and delivery as a curative means in postpartum hemorrhage, inasmuch as these cover the only conditions in which the placenta is a factor in the production of bleeding. When, however, in much the largest class of cases, the flooding is due to uterine *inertia*, the relation of the placenta to the flooding is entirely different. Here the fault lies exclusively with the uterus, or rather with the nerves supplying the uterus; here there is no such imperative demand for active interference with the placenta, since it is simply a passive element in the case. The inertia is the thing to be gotten rid of and not the placenta, as is often taught and practiced. In order to accomplish this, ergot, friction over the abdomen, ice, inside and outside of the uterus, and other active measures should be resorted to, but forcible removal of the afterbirth is not one of these, unless, as we have said, it be deemed advisable to clear the uterus out, for the purpose of applying per sulphate of iron, vinegar or some other styptic directly to the bleeding vessels. But these remedies are not usually resorted to until many other readier methods have failed.

I have already said that if the placenta is detached, or not preternaturally adherent, it can of itself offer no obstacle to a perfectly safe degree of contraction. If any evidence were

needed upon this point beyond our daily experience in natural labor, the testimony of Ruysch, Wm. Hunter and many others of their school, who adopted the practice of leaving the placenta in utero for hours and days, till nature herself threw it off, without hemorrhage—would seem to definitely settle this point. Again, if the placenta be of normal adhesion, the less the area of surface detached the better, so long as atony continues, because the bleeding surface is at a minimum. Here the attachment of the placenta, so far from being an evil requiring to be broken up, is a fortunate circumstance, and is due to the inertia—to the fact that the uterus has not contracted sufficiently to slide it off. Forcible detachment with the hand under these circumstances is certainly very questionable practice, and should not be resorted to without due consideration.

Yet there are few of our text books that contain any clear instruction on this point. The late Prof. Bedford, however, has with his usual vigorous style, presented this matter in its true light. In his lecture on the management of "external hemorrhage," he says, "Flooding may occur when the placenta is completely or partially detached and yet within the uterine cavity, or it may occur after this mass has passed from the organ. It is a very singular fact that many practitioners imagine the *sine qua non* of success in the management of hemorrhage to be the removal of the placenta, and hence in these cases, the very first thing attempted is to extract this body, under the impression that with its delivery the flooding will cease. There never was a more perfect delusion. Why, gentlemen, the afterbirth in strict truth has nothing to do with the hemorrhage; it is not a bleeding surface, and whether it be within or outside the uterus is a matter of utter indifference, so far as the great object is concerned—*the inducing of uterine* contraction. The practice is founded upon vague and indefinite notions with regard, in the first place, to the true cause, and secondly to the true source of the hemorrhage."

It only requires a little reflection, I think, to convince us that this position of Dr. Bedford is unimpeachable, and that the successful treatment of post partum hemorrhage must depend upon rational therapeutics, addressed *to the cause*. In the hands of a skillful and vigilant physician it can scarcely ever occur that anything like a fatal hemorrhage could depend upon any other cause than *inertia uteri*. Other causes may induce severe bleed-

ing, but rapidly fatal hemorrhages are almost always the result of uterine atony. This is the class of cases which demand prompt action, clear judgment and discrimination, and these teach us that our efforts should be directed to the induction of uterine contraction, rather than in exciting our already nervous patient in unnecessary haste to detach and deliver the innocent placenta.

I am satisfied, Mr. President, that there is a great deal of misjudgment and false reasoning in connection with the management of the placenta in such cases. *Because* the hemorrhage frequently ceases on the forcible delivery of the afterbirth, such removal is credited with the result, but, as I have already said, the *post hoc, ergo propter hoc* argument in this case does not bear critical analysis; for when this happens, the stoppage of the hemorrhage is simply an incident due to the stimulation of the uterine walls during the act of detachment and delivery of the placenta. But since the presence of the placenta did not cause the inertia in the first place (exactly the reverse being the case), its detachment (when normally adherent) and delivery cannot possibly, within themselves, cure it. The same amount of intrauterine stimulation would produce the same result independently of the placental delivery. And, on the other hand, if the inertia be profound, and is dependent, as is often the case, on extreme nervous exhaustion, the manual detachment of the placental mass will possibly fail to awaken contraction; in which event the hemorrhage will be increased in proportion to the number of additional uterine sinuses thus laid open.

The fact is that the more we study the pathological physiology of these cases, the more apparent does it seem that the real cause of the *inertia* lies hidden back in the deeper recesses of the nervous system; causes that we have not yet fully fathomed, but which are doubtless intimately connected with the secret springs of the female economy. These must needs be touched before we reach the true physiological remedy, either in the way of prophylaxis or treatment.

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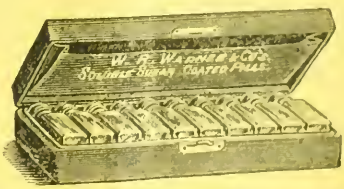

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DISCUSSION.

Dr. W. Johnston—Said he could not accede to the second proposition, that the placenta partially detached was not actively concerned in hemorrhage. The placenta was partially connected with the uterus, and the circulation was going on; it must produce hemorrhage. In that condition, the accoucheur should use all means possible to bring about uterine contraction. He might introduce his hand, and stop the flowing at once. He could not be justified in that course of action unless he was satisfied it would produce uterine contraction. The next best course was to stimulate the nerves running through the uterus; ergot was useful; cold applications would bring about contraction.

Dr. G. J. Engleman—I am glad to hear that paper. I think as a rule, postpartum hemorrhage and retention of placenta are consequences of uterine inertia. It is true most text books say, "Remove the placenta," and some writers not in the text text books insist upon the instantaneous removal of the placenta in postpartum hemorrhage, provided the hemorrhage occurs before the removal of the placenta. It is generally true that after the removal of the placenta, the hemorrhage ceases. But why? Either the placenta is removed by contractions of the womb, excited externally, or the hand is introduced into the womb, and it excites uterine contraction. If we apply pressure externally, we may excite contraction, we may introduce one hand, and with it compress the uterine fundus. That is an excellent way of stimulating contraction. Our object is to stimulate uterine contraction and the placenta is removed. I suppose there are cases where the retention of the placenta will cause hemorrhage, possibly where there is adherent placenta, and yet if the uterus contracts well, there will be no serious hemorrhage.

Dr. W. L. Barret—These are innovations of Dr. Coles; the idea of leaving the placenta in the uterus with impunity for an indefinite time, or the idea of not removing the placenta, for the prevention of hemorrhage, or the checking of hemorrhage. I wish there was more time to discuss the subject. I think it deserves to be fully discussed. If the uterine contraction is at all firm, the vessels are sufficiently ligated to stop the flow of blood.

Anything that prevents contraction of the uterus, will increase the chances of hemorrhage, and so long as the placenta or any mass remains there, preventing contraction, the danger of hemorrhage will be increased. It does not matter whether it is a tumor or something adherent to the organ; it increases the danger of hemorrhage. I do not pretend to dispute that there are a great many cases in which the placenta is probably partly separated from the uterus, and yet there is no excessive hemorrhage. The placenta is almost always in part separated, as in the last contractions, that expel the child.

DR. ATWOOD — I am satisfied that in all cases of postpartum hemorrhage there is a failure on the part of the uterus to contract. During the discussion I have recalled certain cases that fully convince me of the correctness of Dr. Barret's position. You must have firm contraction to close the mouths of the blood vessels. I remember one case in which after delivery, following the doctrine I had then read, to immediately remove the placenta, I carried my hand up, found the placenta within the os, put my fingers around it and removed it, supposing I had removed the entire placenta. I did not, as I think every obstetrician should, examine to see if any portion was left. My patient was attacked with an alarming hemorrhage. I immediately made firm compression on the uterus and applied cold water. I gave her brandy and water, but all to no purpose. It occurred to me I might have left a portion of the placenta in the uterus, introduced my hand, removed the remainder of the placenta, only about two inches, and the trouble ceased immediately. My patient made a good recovery. I wish to cite another case which has often caused me thought and trouble. I was engaged to deliver a certain woman, and being out of town, a neighboring midwife was called in. I visited the house and asked if the afterbirth had been removed; was told it had been. I gave the usual directions for management and left. I heard nothing more of the case for seventeen days, and was then sent for. I found her flooding violently. I gave her ergot and stimulants, but to no purpose. Then I introduced my hand and found a tumor near the fundus. With some difficulty I detached a large saucer full of what appeared to be placenta. The hemorrhage immediately ceased; there was no further trouble. These are proofs positive to me that the uterus could not contract as long as there was a portion of the afterbirth unremoved.

Dr. Coles — I am sorry that I have read this paper so near the hour of adjournment. [He read extracts from his manuscript and asserted that he did not advocate the retention of the placenta for an indefinite time, he said it need not be delivered as a necessarily curative method in flooding.]

The further discussion of the subject was postponed.

Saturday, February 7, 1880.

Diabetes.

Dr. T. F. Prewitt — I have a case I have thought of reporting, as it may be of some interest. The patient is an old gentleman, 75 years of age, who for the last six or eight years has suffered from diabetes, the amount of sugar in the urine being very large. In spite of the fact that he had this large amount of sugar in his urine, his general health kept pretty well. Some twenty-three months ago he was taken with a neuralgic pain in his right leg, which came on periodically with the regularity of clockwork; in the evening the pain came on, and would be very intense all night. Next morning at seven o'clock it would leave him, and he would be free from it during the day. He suffered intensely from this. About eight months ago it left the leg and appeared in the arm and shoulder of the same side as the leg that had been affected; he still had some pain about the foot, but not the intense pain in the leg that he had before. Some four weeks ago I was called to see him, and found him with *herpes zoster* on the right side of the head and face, coming to the middle line with an accuracy as though it had been mapped out, and covering the forehead, cheek, and also the roof of the mouth. He had a deal of pain from it. I felt some apprehension about his eye. It not unfrequently occurs that the eye has been lost as a result of the disturbance of the nutrition; the sloughing of the cornea destroying the eye. I watched it with a good deal of interest for that reason, but I told him it would run a course of about two weeks, and then get well of itself, whether I did anything for it or not. That proved to be the case. There has been a great deal of neuralgia in the supra-orbital nerve, from which he suffers a great deal now. There are some other features of interest. He has always been very

constipated, perhaps going for a week at a time without any action of the bowels. He has had for years a heavily coated tongue at all times, never, however, a very thick fur. Notwithstanding this state of affairs, his general health appeared to be good. He had a well developed physique—rather fleshy; still he had this constant coated tongue, an obstinate constipation and rather a poor appetite. I have heard him say he was in the habit of eating because it was necessary he should eat, not because he had a desire for food. I think there is some connection between the heavily coated tongue and constipation, and the diabetic condition. We know that the exact origin of diabetes is not yet settled, but there is some mal-assimilation beyond it all. The case is an interesting one from all the facts connected with it, especially on account of the amount of endurance of the old gentleman. He is gradually wearing out. I think he will never be able to get out of bed again. For the neuralgia in the leg he has tried a great many remedies. I have prescribed thirty, forty or fifty grains of quinine. Sometimes when he took fifty grains during the day he had no neuralgia that night. I have never got him to take quinine in the heroic doses I would like him to take it. He has a great aversion to it, and complains that it affects his hearing. If the pain was in both limbs I should have suspected a special origin, but it was unilateral and periodic, coming on in the evening at about seven o'clock with perfect regularity, and lasting till seven in the morning. He would be comfortable in the day; he has been in the habit of taking morphine to relieve relieve him.

DR. WESSELER asked if there was any emaciation.

DR. PREWITT — More or less. I cannot say there has been much till the last few months. He imagines he is now free from diabetes. Formerly he had his pants stiffened up with sugar. I tested the urine, and there is sugar in it, but far less than two years ago.

I look upon the neuralgia as the result of a debilitated condition of the system. The difficulty in controlling it is that his constitutional vigor is already undermined. It is doubtful whether any kind of treatment would have cured him for the very fact that the constitution is already undermined, and the vigor of his system broken down, and probably no chance but to alleviate it. He has been rather an abstemious liver.

Dr. Hurt—Has he the usual thirst of diabetic patients?

Dr. Prewitt—He has not now, and while he suffered most from diabetes he was not under my care.

Dr. Hurt—There are two facts that are quite singular in this case, namely, the appetite has been poor, and at present, so far as the doctor is able to state, the thirst has not been very urgent. I believe in the majority of cases of diabetes they are remarkable for their disposition to gourmandize, and for their thirst for liquid. In regard to the pathology of diabetes, it always seemed to me it was perhaps the most obscure of any disease to which the human body is subject. It has its origin in the remote inception of the vital activities, where it is difficult to decide what particular system or organ, or what part of the organism is originally at fault. I am inclined to the opinion that it is a neurosis rather than due to mal-assimilation. It is true that mal-assimilation is present and is a prominent feature of the disease, but am inclined to think that this is owing in great measure to perverted innervation.

Dr. Wesseler—I have seen and treated four cases of diabetes; two of them terminated fatally. Of the four there was not a single case that had the coated tongue that Dr. Prewitt mentioned, and I think that the coated tongue would only be present in old men, for in the case of young men they are generally greedy eaters and drinkers, and wear out before they are old. I think, when a man is seventy-five, he may pass a good deal of sugar, and not wear out so soon as a young man. In the four cases I saw, there was intense redness of the tongue, great thirst and considerable hunger; they could not be satiated with anything. The two last cases passed from under my observation; I presume one of them has terminated fatally by this time. Of all the remedies I used to relieve the sufferer of thirst, the best was bi-carbonate of soda and opium, giving ten to fifteen grains of bi-carbonate of soda and one of opium, three or four times a day. I do not think there is any cure for diabetes. I treated one patient, who seemed to get well, gained thirty pounds of flesh, and made arrangements to go to Europe, but suddenly died before leaving.

Removal of the Placenta.

Dr. Coles—I trust the society will permit me to refer to the subject under discussion at the time we adjourned last Saturday

night. I read a paper at a late hour, and some points made in my paper I think, perhaps, were not clearly understood by some gentlemen. I should be sorry, indeed, if any member of the society should think I advocate absolute non-interference with the placenta in all cases of postpartum hemorrhage; such was not the position I took at all. The object of my paper was to point out the loose and imperfect manner in which most of our text books deal with the subject of postpartum hemorrhage, especially as regards its relation to the placenta. My object was to point out the true causes of postpartum flooding, and to draw attention to the fact that in many of the most dangerous of these cases the placenta is not an *active agent* in its production, and hence, that it is important before proceeding to deliver the afterbirth to determine whether the fault lies with it, or whether it is not due to uterine inertia. In which case, I contended, that harm rather than good might result from premature detachment and delivery of the placenta from a flabby, uncontracting uterus.

The Use of Pessaries.

DR. WM. JOHNSTON drew a diagram on the blackboard illustrating the relative positions of the uterus, rectum and bladder. He said the subject upon which he desired to speak was an old one, on which he differed from many higher authorities. He expressed his belief that the pessary did harm rather than good; and called attention to the comparative distances between the arch of the pubes, and the sacrum, in front of which was the rectum, arguing that if the pessary was used as a fulcrum, it could not, according to the laws of physics, do the good that was claimed for it. In his opinion the only pessary that could do good was the *stem* instrument of Simpson, introduced through the cervix into the body of the womb; that would hold the womb in position. No ring pessary could avail anything; he had abandomed them entirely. When there was relaxation of the vaginal walls, he used carbolic acid and sulphate of zinc, which tended to contract its tissues, and thereby give some support to the body of the uterus.

PRESIDENT MAUGHS said that Dr. Johnston had opened an interesting subject, upon which he was not quite alone in his views, since some respectable authorities held similar tenets.

Dr. Johnston said that he did not claim that the diagram was scientifically drawn.

Dr. G. Hurt—I was going to take exception to the diagram upon the ground that it is erroneous, and does not represent the normal position of the parts, which seem to be out of the true line. Instead of the uterus being a little posterior to the vertical line as represented in that diagram, it is just the reverse. And while it is true sometimes the application of the pessary increases the risk of flexion or misplacement of the uterus, producing that which it is used to remedy, yet I am not disposed to reject that instrument entirely. Under all forms and under all circumstances, I have used the pessary very moderately in my professional career, and I have to say that in some cases I was satisfied it was not useful, but I have introduced it in other cases in which the patient seemed to express satisfaction. I treated a lady some few years ago, who was subject to very painful and excruciating paroxysms of dysmenorrhœa, and, with other remedies used in that case, I applied a pessary. As far as the examination revealed, the trouble arose from a retroflexed and tortuous cervix. This lady entirely recovered, and has since borne children.

Dr. Fairbrother—Do I understand Dr. Johnston to say that it is the stem pressary which is the greatest humbug ever foisted on mankind?

Dr. Johnston—No, sir; the only one that can do any good is Simpson's stem pessary, and it should be watched with a good deal of care. All the others I condemn.

Dr. Fairbrother—Mr. President, this is the same line of argument that was followed in a paper which was read here last fall. In this paper, and I believe in some of the remarks which followed it, pessaries were denounced as a dangerous imposition upon credulous women, and as mere toys to tickle the fancy.

It was certainly with a feeling, something akin to pain, that I listened to this sort of talk, because at that time I had under my care a remarkable illustration of the good effects following the use of the pessary. It was in the case of a woman who made her living by washing. The uterus was prolapsed so that the os projected beyond the external labia. This condition had been

growing for years, until at last she was disabled from work, and she sought relief. The uterus was replaced, a simple ring pessary being introduced into the vagina, and the woman returned to her work entirely free from the pains and forebodings that were rapidly wearing out her life. This is only one case; you have all seen similar ones. I am speaking only of procidentia—saying nothing of the vast amount of good to be obtained by their skillful use in the various flexions and versions. There certainly can be no doubt of their utility in such a case as the one I have instanced.

DR. COLES—I think there is no doubt but that pessaries are often employed when they are really of no advantage, and sometimes when they are a positive disadvantage to the patient; but there are many cases, as remarked by Dr. Fairbrother, where a well fitted pessary is a *sine qua non*. I am reminded of a case, where during the past week, I have enabled a woman to resume her daily duties whose life was before that a burden to her on account of prolapsus with retroflexion. In order to derive satisfactory results from a pessary, it must be judiciously selected and properly applied. The same instrument does not suit every case, nor does every case of womb trouble demand a support of this kind. Dr. Johnston is in error both in his premises and conclusions; his drawing incorrectly represents the relations of the parts, and he is entirely at fault as to the manner in which the ordinary pessary subserves its purpose. With such crude views of their application, it is not remarkable that he has become discouraged with their use. The doctor represents the ordinary lever instrument of Smith and Hodge as resting upon the pubes anteriorly and passing upwards and backwards in a straight line to the hollow of the sacrum, the upper and posterior extremity pressing directly against the rectum. This however, is not as it should be. The pessary, like the forceps, must be adapted to the curves of the vagina and pelvis, as without these, and without perfect adjustment, either instrument is capable of harm.

The normal anatomy of the pelvic organs must be kept clearly in mind, as well as the mechanism of uterine displacements, before any attempt is made at reposition and retention of the womb by mechanical means. I prefer the pessary of Hodge or Smith to any others. In both of these instruments

there is a double curve, well adapted to the natural curve of the vagina. This curve is essential, not only to keep the instrument in place, but to the comfort of the patient; resting against the pubes in front, it projects backwards and upwards, not directly against the rectum, as Dr. Johnston asserts, but curves upward until its lateral bars lie *parallel* with this organ, while the superior crossbar of the pessary rests in and supports the vaginal fold reflected between the rectum and uterus posteriorly. Thus adjusted, and if of proper length, the posterior pressure of the instrument, instead of being confined to one point, is shared partly by the rectum, but chiefly by the superior-posterior *cul-de-sac* of the vagina. The instrument, thus placed, has no absolute fixed point of rest posteriorly, but slides gently up and down with every motion and respiration of the patient. Great judgment and discrimination should be exercised in selecting pessaries; sometimes a number have to be tried before a proper fit is secured. To act comfortably and physiologically it must have the proper curve; if too short it is worthless; if too long it is painful and may become dangerous.

My experience with the stem pessary has not been large, but from what I know of it, when I have occasionally employed it for amenorrhœa, I would unhesitatingly condemn the instrument as impracticable and hurtful. As to anteversion pessaries, I have never seen one yet that would accomplish and secure the desired end. It should be remembered, however, that no pessary is intended to correct a malposition of the uterus by its own inherent power. Its province is to assist in holding the uterus in its proper place, *after* the physician has reposited it. This step should always precede the introduction of the pessary, and should be thoroughly done, a point which is frequently overlooked both in debate and in practice.

Dr. T. F. Prewitt—I think Dr. Johnston's condemnation of pessaries in general is too sweeping, and his indorsement of the stem pessary too generous. I agree with him in the fact that in many cases we find great difficulty in supporting the uterus with the pessary proper. If you take a retroflexed and retroverted uterus, I think all of us will agree that the pessary answers our purpose very indifferently in carrying it up and forward. By reason of this very insufficiency in the leverage we bring to bear upon it, the fundus under these circumstances is probably thick

ened, congested and the pessary does not reach high enough to lift up the weight of the body of the uterus and carry it forward. But in a simply retroverted uterus that is disposed to tip back when one places it in position, I think in a majority of cases a well adjusted pessary would retain it there. Where we have a retroflexed uterus, Dr. Johnston would dispense with the pessary, but he does not tell us what he would do. As to the stem pessary, I think it a dangerous instrument.

DR. BOND said that he had no doubt but that pessaries served a good purpose. He was satisfied that pessaries had their greatest application in retroversion rather than in retroflexion. The instrument must be adapted to the curvature of the vagina. It must be such that when the force is exerted upward it may be between the uterus and rectum, neither too much against the one nor the other. The instrument is kept in place and assisted in its work as a supporter by the natural contractility of the vagina, which is normally a closed canal, its walls possessing decided muscular contractility, and these muscular fibres so arranged that their tendency is to contract from below upwards, thus exerting an upward pressure upon the pessary which is transmitted to the uterus.

DR. C. W. STEVENS condemned the use of pessaries when they had been employed for the purpose of correcting any other malposition of the uterus than prolapsus. He did not believe that the mere introduction of any of the Hodge modifications of pessary could correct even temporarily any malposition; that it might for a short time hold the uterus in a certain position, he could not dispute, but that it corrected a malposition he very much doubted. The fulcrum or lever rested on an unstable foundation; the rectum was full at one time and empty at another. He conceded that in some cases the ring pessary had afforded great relief, but failed to recognize the utility of Hodge's instrument.

Book Reviews.

A System of Midwifery. Including the Diseases of Pregnancy and the Puerperal State. By Wm. Leishman, M. D., Regius Professor in the University of Glasgow, Etc. Third American Edition. Revised by the Author. With Additions by John S. Parry, M. D. [Henry C. Lea, Publisher. Philadelphia, 1879.]

When the work of Prof. Leishman made its first appearance, six years since, under the able editorship of the late Dr. J. S. Parry, of Philadelphia, it was well received by the American profession, and has met with a large sale. In a somewhat extended review, which appeared in the May number of this Journal (1874), we gave this work a full and fair criticism, pointing out what we then considered many of its merits and defects—a good many of the latter being doubtless incident to the preparation and printing of a new volume. The *second* edition, which came out some years since, was a great improvement on the *first*, and the present issue, although it has lost the careful and learned supervision of its lamented American editor, is still better.

The notes and additions of Dr. Parry, so essential to the American student, are retained in the present edition. The author states in his introduction that whilst he considers these addenda for the most part valuable, he does not commit himself to all of them. It is to be regretted that he does not do himself the justice to incorporate many of these into the body of his book, for they undoubtedly embody some of the most advanced and valuable facts in midwifery. For instance, while we can readily understand why our English cousins, from long experience and habit, still adhere to the custom of placing the parturient on her left side instead of on her back, as we prefer, we cannot conceive how they should not only prefer the inferior forceps, but that their bigotry should lead them to utterly ignore all instruments of American manufacture. This is a matter too important to be decided by prejudice, and we are quite sure that no practical man, after trying the various patterns of forceps, can fail to award the palm to America in this respect. We have frequently delivered women, with ease, by means of Hodge's forceps, where repeated trials with English and Scotch instruments had signally failed.

Taking the author's work throughout, it must be confessed that he has presented his readers with a very thorough exposition of the entire practice and science of midwifery. The chapters are all fully and conscientiously written, and set forth in a

pleasing and easy style most that is worthy of note on each subject treated of. The scientific part of the work is exceptionally excellent. Many practical points, however, are not handled in a manner best calculated to meet the wants of students. In this respect Leishman's treatise is not as well adapted for a college text book as some others, and for the reason that on many important questions there is rather too much non-committalism about it; there is a lack of that individualism in points of practice which is so characteristic of Meigs, Dewees, Denman, and many older writers, the effect of which is to leave the inexperienced in doubt, after all that is said, as to what is the proper course to pursue in any given emergency. The fact, however, that the author has compiled the leading theories on most disputed points, renders his work peculiarly suitable as a book of reference for practitioners and more advanced students, who are presumed to be competent to form a judgment of their own. As an illustration of this criticism it is only necessary to read the chapter on *puerperal eclampsia*. In treating of the pathology of this disease the author gives a *resumé* of the various and conflicting views that have been entertained by the most eminent authorities (excepting that he touches lightly on the neuropathic theory), but he fails to emphasize his own opinions, either as to pathology or treatment; certainly not sufficiently so, to lead the inexperienced reader to feel that he has a sure footing to stand upon in the face of such an appalling emergency as this. The whole chapter, while it contains a great deal in doctrine and advice to challenge our approval, is nevertheless calculated to bewilder and confuse the student. In the first place the writer starts out with a definition of puerperal eclampsia, rather broad for practical purposes. He says: "Under the designation of puerperal eclampsia are included not only such instances of the malady in question as are manifested during the puerperal period, but all cases, without exception, which are observed in the course of pregnancy, during labor or after delivery." According to this, if a woman has a fit at any time between the moment of her first conception and her death, even though this should be of old age, it might be denominated puerperal eclampsia.

Under the head of *treatment* the author hedges his advice about with so many *provisos* that it is really difficult to know what *he* would do in any given case. He extols the lancet in one sentence, and in the next so strongly cautions the student against its use as to confuse and frighten him. On the whole the reader is left to infer that blood-letting is not held in high esteem. He speaks of it as the "*old method*," and compares it with the treatment by anæsthetics, quoting the thesis of M. Charpentier, in which the latter claims a mortality of 35 per cent. in bloodletting, against only 11 per cent. under anæsthetics. Nothing however is said about combining the two methods, so essential

in a large proportion of cases, unless indeed such a construction can be placed upon the following vague sentence, which is unfortunately typical of too many throughout the work, wherein the author says (page 665), "We must carefully avoid moreover the danger of adopting any particular method of treatment to the exclusion of others."

The chapters on *puerperal fever* are among the best, and give a very clear and satisfactory account of this interesting disease. The author, however, strangely omits any mention of the fever thermometer, so useful in all the stages of this disease, not only as an early aid to diagnosis, but in taking our bearings as to the progress of the case.

Although there are many subjects on which the author has failed to make his own views sufficiently explicit to satisfy those who read this work expecting to find straightforward practical teaching, it is nevertheless a most complete and valuable compendium of nearly all that is new and authoritative in midwifery. And, although for the reasons we have stated, not the best book for new beginners, it is one of the most valuable additions which the obstetrician could make to his library. The publisher, as is his habit, has done his part faithfully.

W. Coles.

The Pathology and Treaement of Venereal Diseases. By Freeman J. Bumsted, M. D., L. L. D., etc. Fourth edition, revised, enlarged, and in great part rewritten by the Author and by Robert W. Taylor, A. M., M. D. [H. C. Lea, 1879]

The success achieved by the preceding edition of this work was well deserved. Every American practitioner has had cause to thank the author for correct information of the nature and treatment of venereal maladies. The observation, investigation and treatment of syphilis, have of late years acquired new impetus throughout the civilized world, and as a result there accumulated a vast fund of knowledge which has been epitomized and utilized in the preparation of Bumsted's fourth edition. No man in America was as well fitted to perform this task as the author whose individual study and research eminently qualified him to weigh and sift the experience of others, so as to unerringly present the state of our knowledge upon the subject. And if anything were wanting to consummate this desisrable result, the difficulty was satisfactorily overcome by the conjoined assistance of Dr. Robert W. Taylor, whose work on bone syphilis issued in 1875, gave him prominence as a syphilographer.

A leading fault in many medical works of the present day is elaborate theoretical dissertations conveyed verbosely in transcendental style, bewildering to the average medical mind and guiltless of practical and useful instructions. This objection does not apply to the work under review. It is a genuine clinical record condensed and concise, yet presented in a delightfully

flavoring style which arrests and enchants the attention, while it sharply conveys the lesson. The idex is complete and in a moment one can obtain the suggestine idea or information desired. LeGrand Atwood.

Notes on the Treatment of Skin Diseases. By Robt. Lieving, A. M., M. D. Cantab, F. R. C. P., London. Lecturer on Dermatology to the Middlesex Hospital Medical School. [Wm. Wood & Co., New York, publishers.]

This small work on the diseases of the skin admirably answers the purposes for which it was written, viz: to furnish students and general practitioners with a concise and correct account of the etiology, diagnosis and treatment of cutaneous diseases. It is a truthful and reliable reflex of modern dermatology in a very condensed and practical form, and the treatment recommended for the various forms of diseases is as good as any that have as yet been suggested. The long list of valuable and well selected formulæ at the end of the work has been collected from personal experience and the writings of numerous distinguished authors, and is invaluable to persons who only occasionally have to treat diseases of the skin, and consequently cannot be expected to understand the appropriateness of different remedies and combinations of remedies as thoroughly as specialists and men of more extensive experience do.

Thos. Kennard.

Diseases of Women. By Lawson Tait, F. R. C. S. Second edition, thoroughly revised and enlarged. Specially prepared for "Wood's Library." [Wm. Wood & Co., New York, 1879, pp. 192.]

This is one of the "dollar series" of publications for which the profession has to thank the enterprise of Messrs. Wood & Co., by whose liberality a valuable library can soon be accumulated by every member of the profession, no matter how restricted his resources are, at a comparatively trifling cost. Dr. Tait's work on the diseases of women, has long been known as a valuable contribution to this branch of medicine, and while not as full as some others, it is nevertheless written in a very concise and clear style, in which the author imparts his own views and ample experience with commendable perspicuity. As he states in his preface, the book is intended to impart practical information, founded on individual experience. In these days of book making, when so many authors fill their pages with undigested excerpts, and manage to dodge the responsibility of recording positive views of their own, a treatise like that of Dr. Tait should be appreciated. It touches, too, on many subjects not usually included in works of this character, rendering it, on the whole, an exceedingly valuable contribution to our literature on the subject of female diseases. W. Coles.

Books and Pamphlets Received.

A Practical Treatise on Urinary and Renal Diseases including Urinary Deposits Illustrated by Numerous Cases and Engravings. By William Roberts, M. D. Third American, from the Third Revised and Enlarged English Edition. 8vo. pp. 625. [Philadelphia: Henry C. Lea, 1879.]

The Advantages and Accidents of Artificial Anæsthesia: A manual of Anæsthetic Agents, and their Employment in the Treatment of Disease. By Laurence Turnbull, M. D., Ph. G. Second Edition, Revised and Enlarged, with twenty-seven Illustrations. 12vo. pp. 322. [Philadelphia: Lindsay & Blakiston, 1879.]

American Health Primers. Brainwork and Overwork. By Dr. H. C. Wood. [Philadelphia, Presley Blakiston, 1012 Walnut Street. 1880.]

On the Internal use of Water for the Sick, and on Thirst. A Clinical Lecture at the Pennsylvania Hospital, October 25, 1879. By J. Forsyth Meigs, M. D. [Philadelphia: Lindsay & Blakiston. 1880.]

Priority in the Anæsthetic use of the Bromide of Ethyl. By R. J. Levis, M. D. From the "Philadelphia Medical Times," Feb. 14, 1880.

Hygiene and Education of Infants; or How to Take Care of Babies. By the Société Française D' Hygiene, Paris, France. Translated from the French. By Geo. E. Walton, M. D. [Cincinnati: Robert Clark & Co. 1880.]

M. Paquelin's Thermo-Cautery, with Willson's Antithermic Shield, in Epitheloma of the Cervix Uteri. By H. P. C. Willson, M. D. Baltimore, Md.

Malignant Degeneration of a Fibroid Tumor of the Uterus. Large False Aneurism in the Substance of the Growth. Drs. Albert N. Blodgett and Clifton E. Wing, Boston.

Responsibility Restricted by Insane Delusion. By T. L. Wright, M. D. Bellefontaine, Ohio. [Reprinted from the Cincinnati Medical News, Nov., 1879.]

Sanitary Organizations of Nations. By Henry I. Bowditch, M. D. [Reprinted from the Boston Medical and Surgical Journal]. Cambridge: Printed at the Riverside Press. 1880.]

METEOROLOGICAL OBSERVATIONS.

By A. WISLIZENUS, M. D.

The following observations of daily temperature in St. Louis are made with a MAXIMUM and MINIMUM thermometer (of Green, N. Y.). The daily minimum occurs generally in night, the maximum at p. m. The monthly mean of the daily minima and maxima added and divided by two, gives quite a reliable mean of the monthly temperature.

THERMOMETER, FAHRENHEIT—FEBRUARY, 1880.

Day of Month.	Minimum.	Maximum.	Day of Month	Minimum.	Maximum.
1	26.0	33.0	18	28.0	38.0
2	21.5	23.0	19	23 5	33.5
3	25.0	28 0	20	25.0	36.5
4	17.5	35.5	21	34.0	47.0
5	26.5	41.0	22	34.5	64.0
6	25.5	49.0	23	38 5	53.5
7	25.5	36.0	24	44.0	67.5
8	27.5	55.0	25	55.0	59.0
9	26 5	45.0	26	47.0	**68.0**
10	36 5	52.5	27	49.0	66.5
11	44 0	59.0	28	41.0	58.0
12	43.0	46.0	29	20.5	30.5
13	26.0	30.0	30		
14	**14.5**	35.5	31		
15	25.0	8.0			
16	35.0	56.0	Means.	32.0	47.1
17	46.0	61.0	Monthly Mean	39 5	

Quantity of rain and snow, 2.95 inches.

MORTALITY REPORT.---CITY OF ST. LOUIS.

FROM FEBRUARY 8, 1880, TO FEBRUARY 21, 1880, INCLUSIVE.

Ovarian Tumor	1	Exhaust. P'm Lab.	1	Convulsions & Trismus Neonatorum	18
Measles	5	Inanition, Want of Breast Milk, etc.	3	Hydrocephalus and Tub. Meningitis.	1
Syphilis	1	Alcoholism	4	Meningitis & Encephalitis	9
Scarlatina	2	Rheumat'm & Gout	1	Other Diseases of the Brain and Nervous System	3
Pyæmia & Septicæ	4	Cancer and Malignant Tumor	8	Cirrhosis of Liver and Hepatitis	7
Erysipelas	3	Phthisis & Tuberculosis, Pulmon.	34	Enteritis, Gastro-Enteritis, Peritonitis, and Gastritis	9
Diphtheria	7	Bronchitis	8	Bright's Disease and Nephritis	2
Membran's Croup.	4	Senility	8	Other Diseases of Urinary Organs.	
Whooping Cough.	3	Pneumonia	41	Atheroma Arta	0
Diabetes Meli itus.	0	Heart Diseases	5	Placenta Prævia	1
Umbilical Hem'ge	2	Other Diseases of Respir'y Organs	9	Apoplexy	5
Typhoid Fever	3	Osteomyelitis	1	Cyanosis and Atelectasis	
Cerebro Spinal Fe.	0	Marasmus — Tabes Mesenterica and Scrofula	13	Premature & Preternatural Birth	0
Remittent, Intermittent, Typho-Malarial, Congestive & Simple		Aneurism	1	Deaths by Suicide	2
Contin'd Fevers,	6			Deaths by Accid't	7
Puerperal Fevers	2			Congen Defor'ty	9
Diarrhœal Disea's	3			Total Deaths from all Causes.	254
				Total Zymotic Diseases	50
				Total Constitutional Diseases	57
				Total Local Diseases	118
				Total Develop'tal Diseases	20
				Deaths by Viol'ce	9

CHAS. W. FRANCIS, *Health Commissioner.*

TROMMER EXTRACT OF MALT CO.,

FREMONT, OHIO.

FREMONT, O., April, 1878.

DEAR SIR :—It is now five years since we first introduced and began the manufacture of Extract of Malt in the United States. It has been our aim to furnish the medical profession in America with a malt-extract equal to the best German make, and (by saving the expenses of importation), much cheaper than the foreign article can be afforded. For the manner in which our efforts have been appreciated by the medical profession, we desire to express our warmest thanks.

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We do ourselves but simple justice, in stating that our *entire attention* is, and for many years has been, *exclusively* devoted to the manufacture of Extract of Malt for medicinal purposes, and that we give our undivided personal attention to each step in the delicate process by which Extract of Malt of excellent quality can alone be made.

Under these circumstances, it is unreasonable to suppose that the various manufactures of fluid extracts, elixirs, pills, etc., (who attracted by the high reputation of our Extract of Malt), have recently in various sections of the country, undertaken the manufacture of a similar article, should generally succeed in producing it of a quality according to the fulsome praise with which their avertisements are filled. While being perfectly willing to let the reputation of our Extract of Malt rest upon its real merits, we owe it to the medical profession, as well as to ourselves, to give warning against imposition.

It has come to our knowledge that certain articles extensively advertised as "pure" and "genuine extract of malt," are composed chiefly of the substance called GRAPE SUGAR or MALTINE, which, as is well-known, is the product of the action of the sulphuric acid upon starch subjected to a high temperature. This artificial grape sugar of glucose, which is extensively manufactured from corn starch, is now being used in immense quantities, instead of ordinary cane sugar, in the sophistication of confectionery sugar, house syrup, "strained honey" native wines, and canned fruits, and by some brewers in the manufacture of beer and ale. The cheapness of this artificial product of Indian corn, constitutes the chief inducement for this species of substitution for barley malt and sugar cane.

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It is *malt extract* prepared from barley malt combined with the proper proportion of hops, that has been for many years the standard medicinal nutritive employed by the medical faculty of Europe, and especially of Germany. Its value has been established by experience, and its use in the treatment of almost all forms of disease of nutrition is constantly extending. We shall continue to devote the most scrupulous attention to the maintenance of the reputation of our malt extract, by the careful selection of material and by unwearied personal attention to manufacturing details.

Attention is respectfully directed to the accompanying extract from Ziemssen, and also to our circular and testimonials elsewhere printed.

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FROM ZIEMSSEN'S CYCLOPÆDIA OF THE PRACTICE OF MEDICINE, VOL. XVI, PAGE 474 :

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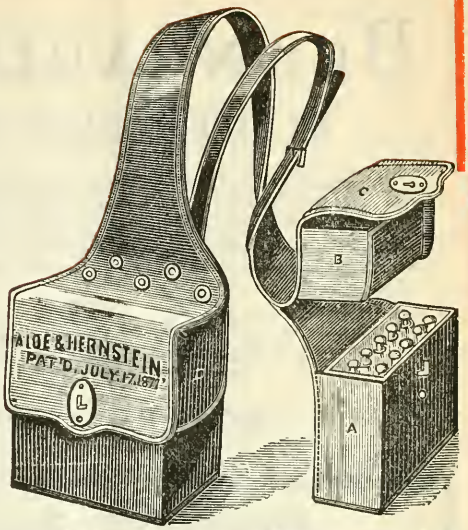

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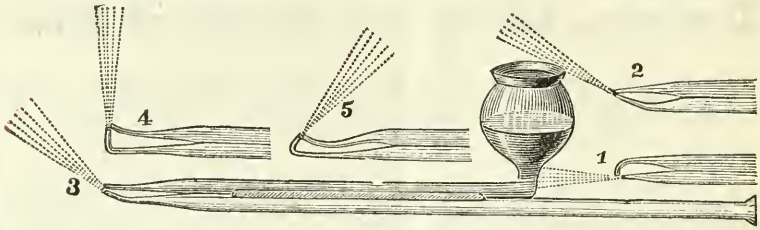

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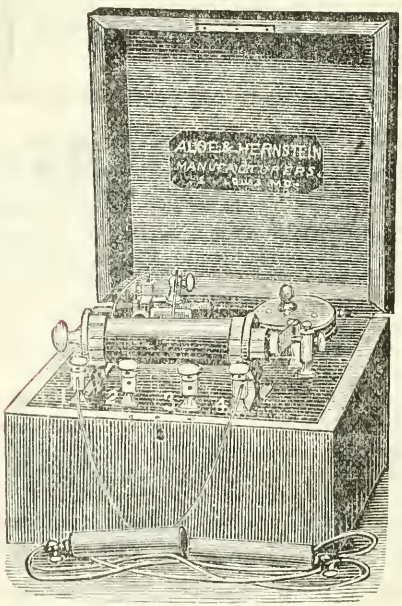

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Index to Advertisements.

PAGE.

Aloe & Hernstein, *Surgical Instruments and Physicians' Supplies*..... 31
Alex. Heburn & Co., *Surgical and Dental Instruments*. .....Facing page 190 of Journal.
Burrington, H. H., *Wadsworth's Uterine Elevator*..... 2
Bedford Alum and Iron Springs Water..... 7
Battle & Co., *Bromidia*..... 11
Caswell, Hazard & Co., *Beef Tonic*... .....1.
Chesebrough Manufacturing Co., *Vaseline*.....Facing page 198 of Journal.
Fongera, E..... ... . 3, 4, 7, 20, 23, 25 & 26
Glycerite of Animal Hypophosphites.....12
Houghton, E. F., *Cosmoline* 23
Kennedy, S. H., *Pinus Canadensis* 6
Landrum & Litchfield, *Iron and Alum Mass*..... 13
Keasbey & Mattlson, *Dextro-Quinine* ....Facing Third Page Cover.
Mackwitz, W., *Engraver* 6
Mellier, A. A., *Wholesale and Retail Druggists*..... .. .. Third Page Cover
New York Pharmacal Association, *Lactopeptine* ... . Second Page Cover
Park, Davis & Co., *New Remedies*.... ....4th Page Cover
Planten's Capsules 2
Reed & Carnrick, *Maltine*..... .... 24 & 25
Robert Shoemaker & Co., *Johnston's Fluid Beef*.... .....Facing page 231 of Journal.
Scheffer, E., *Apothecary and Chemist*..... 5
Schieffelin, W. H & Co., *Soluble Pills and Granules* 8
Spackler & Co., *Surgical, Dental and Orthopædical Instruments*..... 5
Schleiffarth, Charles, *Trusses Supporters, Shoulder Braces, etc*..... 9
St. Louis Type Foundry..... 26
Trommer Extract of Malt Co ....Facing page 214 of Journal and 41
Wm. Warner & Co., *Sugar Coated Pills*..... ... ... Facing page 230 of Journal
Wyeth & Bro . Dialysed Iron 16

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Harvey E. W.....Sparta

COLORADO.

Dicker, L. H.....Saguache
Hover, W. A.....Denver

NEVADA.

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SAMPLE FURNISHED ON APPLICATION.

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THEOPHILUS REDWOOD, Ph. D., F.C.S., Professor of Chemistry and Pharmacy to the Pharmaceutical Society of Great Britain, says, in reference to this preparation:

"UNGUENTUM PETROLEI appears to be obtained from crude Petroleum by fractional distillation and subsequent purification. Products of a similar description have been introduced in pharmacy, and recommended as substitutes for animal or vegetable fats in the preparation of ointments or liniments.

"Among these, the UNGUENTUM PETROLEI is distinguished by its having a firmer consistence and a higher melting point, and in these respects it resembles pure lard, from which it differs in that it is entirely free from liability to become rancid or to undergo any chemical change. It has an amber color and a smooth, jelly-like appearance, and is almost wholly free from odor or taste. It is free from and insoluble in water It is very slightly soluble in rectified spirit, but very soluble in ether, in fixed oils and fats. I am of opinion that its characteristics and properties are such as to render it a valuable adjunct to our *Materia Medica*."

Dr. TILBURY FOX, [London] writes:—"I like the UNGUENTUM PETROLEI very much indeed, and shall not fail to prescribe it."

THE LANCET ANALYTICAL REPORT ON UNGUENTUM PERTOLEI.

From the LANCET, London, October 20th, 1877.—UNGUENTUM PETROLEI.—Semi-transparent, pale yellow in color and perfectly free from offensive smell. We consider it a valuable preparation and hope its use will become as common in England as it is said to be in America."

From the MEDICAL TIMES AND GAZETTE, London, October 20th, 1877.—"The substance introduced under the name of UNGUENTUM PETROLEI is of about the consistence of good lard, has no smell, keeps any length of time, apparently, without becoming rancid, and may be exposed to any kind of atmosphere without undergoing any chemical change. It has long been, we understand, in use in America, where it enjoys a wide-spread popularity, and it will, we doubt not, come into extensive use here also as a vehicle for making ointments, as a demulcent in various disease of the skin, and as a lubricant."

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During the past year we have received nearly one thousand letters from the Medical Profession in this country and Great Britain, referring to the therapeutic value of MALTINE: *their character is indicated by several extracts which we present below.*

BALTIMORE, MD., Jan. 20th, 1879.

We have realized decided benefit in a large number of cases treated in the City Hospital and at the Dispensary connected with it, from your preparations of Maltine. Many persons will welcome them as most efficacious and palatable substitutes for Cod Liver Oil, and as covering a wider range of application.

S. WESLEY CHAMBERS, M. D., *Res. Phys. City Hospital.*

BALTIMORE, MD., Jan. 20th, 1879.

We take pleasure in saying in behalf of your preparations of Maltine, that they have fully come up to the measure of your representations. They have given us the greatest satisfaction. We have used them extensively to the great benefit of our patients.

DAVID STREET, M. D., *Res. Phys., Maternite Hospital.*

LOUISVILLE, KY., July 11th, 1879.

I am using Maltine with Pepsin and Pencreatine in my family, and am exceedingly pleased with its results. Professor Flint, of your city, whom I highly esteem, has been consulted about the case and knows the solicitude I have had about it. The above preparation in Sherry, after meals, has been productive of great benefit. I am using it in the City Marine Hospital, the Kentucky Infirmary for Women and Children, and in my private practice, and am much pleased with the results obtained.

T. P. SATTERWHITE, M. D.

JACKSON, MICH., October, 1878.

In its superiority to the Extract of Malt prepared from Barley alone, I consider Maltine to be all that is claimed for it, and prize it as a very valuable addition to the list of tonic and nutritive agents.

C. H. LEWIS, M. D.

ST. CHARLES, MINN., March 23d, 1879.

In conditions of Anæmia, in convalescence from severe and protracted disease, especially in chronic cases where there is great general debility, and in the enfeebled conditions of aged persons, I have learned to rely on Maltine, nor in any instance have I been disappointed of good results, therein forming *a marked contrast, so far as my experience goes, to preparations of Malt, which I had used previously and had abandoned the use of them when my attention was called to Maltine.*

C. R. J. KELLAM, M. D.

---

26 WEYMOUTH ST., Portland Place, LONDON.
May 30th, 1879.

I am ordering your Maltine very largely.

LENNOX BROWN, F. R. C. S., *Sen. Surg., Centl. Throat and Ear Hosp., etc.*

---

75 Lever St., Piccadilly, MANCHESTER, Jan. 16th, 1879.

I have used your Maltine pretty extensively since its introduction, and have found it exceedingly useful; particularly in cases where Cod Liver Oil has not agreed, have I found the Maltine with Beef and Iron most valuable.

J. SHEPHERD FLETCHER, M. D., M. R. C. S.

Eddle Cross House, Ross, March 8th, 1879.

I am very pleased to bear testimony to the great value of Maltine. I prescribe it extensively and with the best results, specially in anæmic conditions of the system with much stomach irritability, which it seems to allay very speedily.

J. W. NORMAN, M. B., M. R. C. S.

CHEMICAL REPORTS ON MALTINE.

By R. Ogden Doremus, M. D. LL.D.

PROFESSOR OF CHEMISTRY AND TOXICOLOGY, BELLEVUE HOSPITAL MEDICAL COLLEGE;
PROFESSOR OF CHEMISTRY AND PHYSICS, COLLEGE OF THE CITY OF NEW YORK.

New York, April 17th, 1879.

I have visited the works at Creskill, on the Hudson, where Maltine is prepared, and spent portions of two days in witnessing the chemical processes for making the same. I was particularly impressed with the thorough cleanliness observed, as well as with the completeness of the aparatus employed for accomplishing the desired result—from the first treatment of the grains, to the concentration of the liquid product by evaporation *in vacuo*. The operation is effective in extracting the whole of the nutritive constituents of the grains of malted Barley, Wheat and Oats, with but a slight residue, and the most complete method yet devised, with which I am acquainted, for accomplishing this object.

Maltine is superior in therapeutic and nutritive value to any Extract of Malt made from Barley alone, or to any other preparation of any variety of grain. From a chemical and medical standpoint, I cannot commend too highly to my professional brethren this unique and compact variety of vegetable diet and remedial agent, nutritive to every tissue of the body, from bone to brain.

Respectfully, R. OGDEN DOREMUS.

By Prof. John Attfield, F.C.S.

PROFESSOR OF PRACTICAL CHEMISTRY TO THE PHARMACEUTICAL SOCIETY OF GREAT BRITAIN;
AUTHOR OF A MANUAL OF GENERAL MEDICAL AND PHARMACEUTICAL CHEMISTRY.

London, 17 Bloomsbury Square, W. C.

To Messrs. Reed & Carnrick: October 28th, 1878.

Gentlemen:—I have analysed the extract of malted Wheat, malted Oats and malted Barley, which you term Maltine. I have also prepared, myself, some extract from these three malted cereals, and have similarly analysed it, and may state at once that it corresponds in every respect with the Maltine made by myself. As regards the various Malt Extracts in the market, I may remark that your Maltine belongs to the non-alcoholic class, and is far richer, not only in the directly nutritious materials, but in the farina digesting Diastase. In comparison, your Maltine is about ten times as valuable, as a flesh former; from five to ten times as valuable, as a heat producer; and at least five times as valuable as a starch digesting agent. It contains, unimpaired and in a highly concentrated form, the whole of the valuable materials which it is possible to extract from either malted Wheat, malted Oats or malted Barley.

Yours Faithfully, JOHN ATTFIELD.

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MALTINE with Pepsin and Pancreatine.
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MALTINE with Phos. Iron, Quinia and Strychnia.
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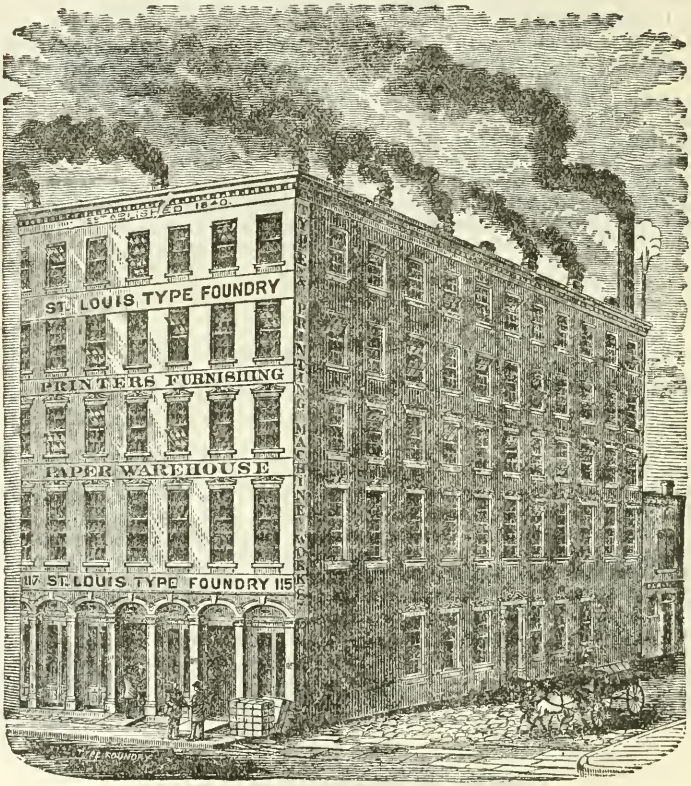

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